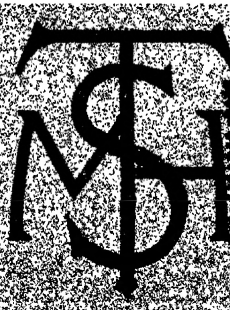
Vol. X. No. 1. November, 1916.

TRANSACTIONS

OF THE

SOCIETY OF TROPICAL MEDICINE AND HYGIENE.



EDITED BY
THE SECRETARY OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

NOVEMBER, 1916. VOLUME X. NO. 1.

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 17th November, at 5.30 p.m. As the afternoon Meetings seem to suit most Fellows, they will be continued until the War is over.

Tea will be provided for Fellows between 5 p.m. and 5.30 p.m.

2. Owing to the fact that the addresses of so many Fellows are still uncertain, and will be so until the War is over, no Year Book will be issued again this year. Fellows will therefore have to rely upon the one issued for 1914-1915 for the present.

EDITORIAL NOTICES

The Editor of the TRANSACTIONS will be pleased to receive short papers to be included under the heading of "Notes and Comments," or "Questions," in addition to longer papers on subjects of Tropical interest, which may be offered for presentation at meetings of the Society He will also be glad to receive books for review

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

DECEMBER, 1916. VOLUME X. NO. 2.

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 19th January, at 5.30 p.m. As the afternoon Meetings seem to suit most Fellows, they will be continued until the War is over.

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

JANUARY, 1917. VOLUME X. No. 3

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 16th February, at 5.30 p.m.

Tea will be provided for Fellows between 5 p.m. and 5.30 p.m.

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TRANSACTIONS OF THE
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FEBRUARY, 1917. VOLUME X. No. 4

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 16th March, at 5.30 p.m.

Tea will be provided for Fellows between 5 p.m. and 5.30 p.m.

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

MARCH, 1917. VOLUME X. NO. 5

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 18th May, at 5.30 p.m.

Tea will be provided for Fellows between 5 p.m. and 5.30 p.m.

2. The Secretaries hope to be able to issue a new Year Book this summer. They specially call the attention of Fellows to this fact, and hope that they will help in the compilation by forwarding at once any changes in their addresses.

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

APRIL, 1917. VOLUME X. NO. 6.

CONTENTS.

SPECIAL NOTICES.

1. The next Meeting of the Society will be held on Friday, the 15th June, at 5.30 p.m.

Tea will be provided for Fellows between 5 p.m. and 5.30 p.m.

2. The Secretaries hope to be able to issue a new Year Book this summer. They specially call the attention of Fellows to this fact, and hope that they will help in the compilation by forwarding at once any changes in their addresses.

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JUNE, 1917. VOLUME X. NO. 7.

CONTENTS.

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TRANSACTIONS OF THE SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

JULY, 1917. VOLUME X. NO. 8.

CONTENTS.

SPECIAL NOTICE.

The next Meeting of the Society will be held on Friday, 19th October, at 5 30 p.m.

Tea will be provided for Fellows between 5 p m. and 5.30 p.m.

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TRANSACTIONS

OF THE

SOCIETY OF TROPICAL MEDICINE AND HYGIENE.

NOVEMBER, 1916.

VOLUME X. No. 1.

Proceedings of a Meeting of the Society held on Friday, October 20th, 1916, at 5.30 p.m., at 11, Chandos Street, Cavendish Square, W., Surgeon-General Sir DAVID BRUCE, C.B., A.M.S. (*Vice-President*), in the Chair.

THE POSITION OF MALARIA IN SANITARY ADMINISTRATION.

BY ANGUS MACDONALD, M.D. (EDIN.), D.P.H., D.T.M.

International Health Commission, Grenada, West Indies.

The work of malaria administration and general mosquito elimination, instead of being relegated to Special Commissions with duplication of authority and indeterminate direction, must be recognised to be part of the routine work of the Health Department wherever that exists; and to take its place in due order of necessity, so far as finance permits, and the mortality and morbidity of the population demand.

In colonies where no health department exists; where a medical department retains the privileges and prestige of the age of cure and cannot conceive the meaning of prevention; or where an enlightened government and an enlightened medical department accept the preventive responsibilities without the necessary hygienic education or knowledge, sanitary improvement is a long way off.

In these the aid of commissions, with specialists *ab extra*, may accomplish something; but undoubtedly real advancement is only to be

secured where modern knowledge is placed at the disposal of a community in the form of an organised health department.

Executive responsibility under varying colonial conditions being retained by "Government" or placed on representative bodies, the first essential is a public health law, upon which may be founded the bye-laws which should deal with every item of sanitation that will come before the executive officers of the health department.

The staff of the health department in its simplest should consist of a Medical Officer of Health specially trained in practical sanitation, holding a degree in science or a diploma in public health, and of necessity in the tropics holding also a diploma in tropical medicine; sanitary inspectors, clerical staff, and labourers.

The number of Sanitary Inspectors depends on the population. For a unit—rural or urban—of 60,000 population, ten sanitary inspectors is a reasonable number. This population represents some 10,000 (urban) to 12,000 (rural) dwellings, and with ten inspectors it should be possible for each premises to be visited and reported on in routine once every three months, or four times a year.

I contend that the value of all public health administration is to be summed up in its domestic inspection and education; and, to get any control over that, the routine visitation is an absolute essential.

The crude notion that a sanitary inspector should go "nosing" around till he comes up against a nuisance is unfortunately, even at the present day, all too common.

Methodical inspection which reports in routine the normal as well as the unusual, the satisfactory as well as the nuisance, with its concurrent educational influence, will avail more than the vicious spy and prosecution system of administration.

The punitive sections of laws are to terrorise the wilful wrongdoer; wise public health laws and bye-laws teach the people what to do, and sanitary inspection is the personal education of the community in general. Wilful wrongdoing is rare; ignorance abounds; a fine of five shillings or a week in gaol will not bring sanitary surroundings to the poor and ignorant.

Of the ten sanitary inspectors the health officer will select his men for duties in which they display special aptitude. In general it will probably be found best simply to give them districts in which they have the entire

control of all sanitary measures, and to select men for special duty as occasion dictates.

A superior inspector may be chosen as a chief sanitary inspector, specially trained or not; but the health officer in any case, if he desires work done, will have to go down to his inspectors individually, go over their records with them, and systematically review their work in the field. *Sic itur ad astra*—only thus. If the health officer expects to control the disease conditions in a tropical community only by plotting curves and pulling strings at his office desk, he had better not attempt work in the tropics. He must see things done.

The Clerical Staff required to do the necessary work for the unit of population assumed will be two, a chief clerk and typist and a junior office boy. The due regulation of their work and apportionment of their duties the medical officer will adjust from time to time.

A Labouring Staff is inevitable in a tropical health department. In the city the scavenging rightly is an important section of the health department. The necessary carts, horses, mules, motor-vans, and all the paraphernalia of tropical scavenging will come under the direct purview of the health office. The manner of their economic engagement will be a matter for the interested authority. Their entire control during working hours will be under the medical officer.

Actual numbers for these need not be cited, conditions varying with population and area, with situation, and with class of labour and transport available.

On the labourer, strange as it may seem, depends the value of the ordinary antimalarial measures in an urban or rural tropical community. An average of a labourer per mile of streets, lanes, gullies and roads may be set down as a fair indication of the necessities of scavenging, street cleaning, gully training, gutter sweeping, and the various odds and ends of practical sanitation in a city. On the co-operation of the whole population depends the value of special antimosquito work, the suppression of Culex and Stegomyia, and sometimes of Anopheles.

In a purely rural community, outside towns, organised control of roadside gutters and other temporary breeding pools may be obtained by having a labourer roughly to five miles of roadway. This figure will vary enormously, owing to the great variety in condition of roads including first-class, secondary bye-ways, bridlepaths and footpaths which are under

authority; and, no matter under what different authorities they may be vested for constructive purposes, their sanitary control should be imposed on the health department. Only thus is there any hope of limitation of mosquito breeding.

The control of roads so far as the engineer is concerned is a matter of construction and æsthetics solely. Gangs of labourers are placed on the roads once or twice, or more often, in a year, to clean bush and train where anopheline breeding often has been going on for weeks or months before under no control; and, as soon as this display of zeal has passed along, the mosquito-breeding conditions begin behind.

The relation of the health department to the constructive department is one of calling on the authority to perform certain works. Here the health department may suggest a length of concrete gutter; there it may request training and levelling and the retention of the natural soil or subsoil drain. Again, it may request filling up, double channelling, and the like. Any or all of these suggestions may be accepted and acted on by the authority. But, accepted or not, it is still up to the health department to control the mosquito-breeding conditions, and prevent nuisance generally.

Then comes in the scheming of the staff: incessant watchfulness—here kerosene, there the brush, again the spade and rubble filling and so on.

Having postulated the details of the department necessary to tackle the whole of the problems of disease in a community, I shall now more particularly describe the special measures of antimalarial administration, including antimosquito measures generally.

These may be summed up as follows:—

1. Systematic Scrutiny of Death Returns.
2. Communication with General Medical Practitioners.
3. Routine Returns, from Institutions, of Malaria and other Mosquito-borne Diseases.
4. General Sanitary Measures.
5. Special Antimosquito Measures directed against:—
    i. Stegomyia in particular.
    ii. Culicines in general.
    iii. Anophelines.

I. SYSTEMATIC SCRUTINY OF DEATH RETURNS

is a substantial base for general sanitary operations. In respect of malaria, whose notification is not politic or practicable, the death returns give knowledge not otherwise readily obtainable, and facilitate the prompt investigation of the family history, residence and surroundings of the deceased. Careful investigation of the surroundings will justify the assumption of the accuracy or not of the cause of death; may reveal neglect on the part of householder or inspector, which allowed anopheline breeding where none should be; will indicate whether infection was local or imported; and the discreet reference to the certifying practitioner should pave the way for greater accuracy in certification.

II. COMMUNICATION WITH GENERAL MEDICAL PRACTITIONERS.

Friendly intercourse with his colleagues in general practice is of inestimable value to the medical officer of health. When, as I have stated above, the compulsory notification of malaria in a tropical community is not politic or practicable ("fever" being a commonplace of diagnosis and "malaria" too commonly the official synonym of "fever") the value of private information of the occurrence of malaria cannot be overlooked.

Concrete examples may aid comprehension of the importance assigned to this courtesy:—

> The health officer is called in consultation to a cottage where ten patients are down with malaria; several comatose. This cottage is within striking distance of a swamp whose reclamation is not feasible. By stocking with fish; kerosening edges, hoofmarks, wallows, etc., malaria had been banished from the area around and no case had been reported for some nine months. For several months kerosening had been intermitted on the ground of economy, and on the evidence of anopheline limitation. In the cottage were two visitors from a country parish, a notorious malaria haunt, who were "spending time" with their friends. The presumption is that they had brought the infection, which the probably clean local anophelines had promptly transferred to the entire family.
>
> Again, in town several cases of malaria are reported in a district where fruit gardening is carried on; irrigation is known to exist and is attempted to be kept under control. Blood examination

establishes the presence of the *Plasmodium falciparum* in several cases. Investigation shews laxity on the part of the Sanitary inspectors of this and an adjoining district. The nearest permanent anopheline habitat is two miles off, and yet at convenient points from these infected dwellings to the swamp, there are found stepping stones or flight pools harbouring, or capable of harbouring, larvæ, enabling anopheline communication to be maintained over the two miles

Practical experience will multiply manifold these examples of the advantage to the health department of the sympathy and assistance of the general medical practitioner or other medical officers seeing patients

III. ROUTINE RETURNS FROM INSTITUTIONS.

It should be possible to obtain from public institutions a return of all cases of malaria and other insect-borne diseases exactly in the same form as the notification of any compulsorily scheduled infectious disease.

Immediate investigation of such notified cases of malaria is of enormous value In many tropical communities, too, it will mean practically all the cases that are found

The facts elicited are .—

The residence and surroundings of the patient ;

The probable source of infection ;

The evidence of fresh infection or relapse ;

Whether malaria or not malaria , and if malaria, the type of parasite ;

The seasonal and annual variation in number of cases ;

The age and sex incidence ;

The respective incidence and virulence of the different parasites.

The assistance towards control that is to be secured from the knowledge thus obtained, need only be mentioned to be appreciated by the average hygienist. None of the points need to be laboured ; but in regard to the actual parasite found and the comparative morbid results observed, a few words may be said, as they are points on which information is lacking and which ought to be obtainable.

There is certain evidence of the actual carriers of *P. falciparum*, *P. vivax*, and *P. malariæ ;* but in different localities different anophelines may be carriers of one and not of others. It might come to be possible

for the sanitary officer to say this or that case must have come from out side his district, as the mosquito carrier of that infection did not breed with him.

Again, there is not sufficient evidence of the morbific influence of the different parasites in spite of their colloquial differentiation into benign and malignant. Sufficient cases should be available in the accumulation of such records to establish the mass morbidity and mortality attributable to the different parasites.

IV. GENERAL SANITARY MEASURES.

It has been asserted above that there is no justification for divorcing malaria administration from the routine of the public health department. In the whole gamut, therefore, of sanitary measures there is general applicability, educational and executive, to malaria. So far as the health officer can get his laws and regulations to cover him, and so far as he may risk, failing these, he should treat the malaria case in general as he would any notifiable infectious disease, emphasising isolation, conducting disinfection, and enforcing rehabilitation of premises and general cleanliness.

There is always a moral and educative value behind much trifling and apparently useless disinfection proceedings.

Of course, the acute vulnerability of plague and yellow fever places them apart from other insect-borne diseases for general administrative purposes; and it is probable that control will be secured over malaria, filariasis, and certain other insect-borne diseases without the necessity to have them scheduled with and subject to the same statutory regulations as plague and yellow fever. But in these others the sanitary officer should go every step the law allows, and the little diplomatic step beyond, in his attempt to secure hygienic isolation (not seclusion) of those who are carriers of these diseases.

The chronic misery they inflict may well be more fatal to any community than the acute disaster of those more dreaded.

V. SPECIAL ANTIMOSQUITO MEASURES.

(a) Stegomyia fasciata.

The limitation of *Stegomyia fasciata* is a matter of incessant domiciliary visitation and education, and the results obtainable from rigid

inspection (without prosecution) are most striking. There are tropical cities at this moment probably harbouring stegomyia on 100 per cent. of premises, and it is open to a keen sanitary staff to reduce that record to 0·5 per cent. in a matter of twelve months.

A general sanitary scavenging clean up is a necessary preliminary, and ample scavenging and removal procedure must be maintained along with house-to-house visitation by capable inspectors, who will instruct householders in the law's demands and demonstrate to them the actual conditions of occurrence and means of prevention.

To detail the innumerable petty sanctuaries of *Stegomyia fasciata* need not be attempted: the creature is purely domestic, and will find breeding situations in any place or thing about buildings and yards that is capable of collecting a tiny drop of water. The wildest spot in which the writer has found *Stegomyia fasciata* larvæ was in rock holes by the sea, within one hundred yards of dwellings. He has also found them in a wild pine, on a tree near his residence, along with larvæ of *Wyeomyia pertinens*.

The sanitary inspector dealing with *Stegomyia fasciata* for any length of time will develop detective abilities of the highest order, but he will appreciate that with all his ingenuity he has still something to learn from it.

One or two examples of unexpected breeding places may be given to illustrate the domestic ubiquity of this insect:—

> The surroundings of an office habitually haunted with Stegomyia are repeatedly searched in vain, and it is almost concluded that the mosquitoes are coming from a distance. Accidentally larvæ are at last found in a small earthenware dish hanging on the wall, and dipped into daily by the copybook brush. Carelessly the dish for some time had not been emptied, but was filled up daily.
>
> In another case a few depressions in the tops of some pillars standing on a ruin were the source of mosquitoes troubling a neighbouring house. Again, after long search, it was found that a chance piling of timber under a heap of lumber caused the retention of rainwater sufficient to permit breeding long after drought had removed all ordinary possibility.

As has been said above the breeding spots of *Stegomyia fasciata* are innumerable; and from experience I believe it is safe to say they are

SWAMPY GROUND (TANTINI, GRENADA).

Left in moist condition after "reclamation" of swamp, and probably now a far greater danger than before from Anopheline breeding. Note the Eucalyptus trees.

LAGOON (GRENADA).

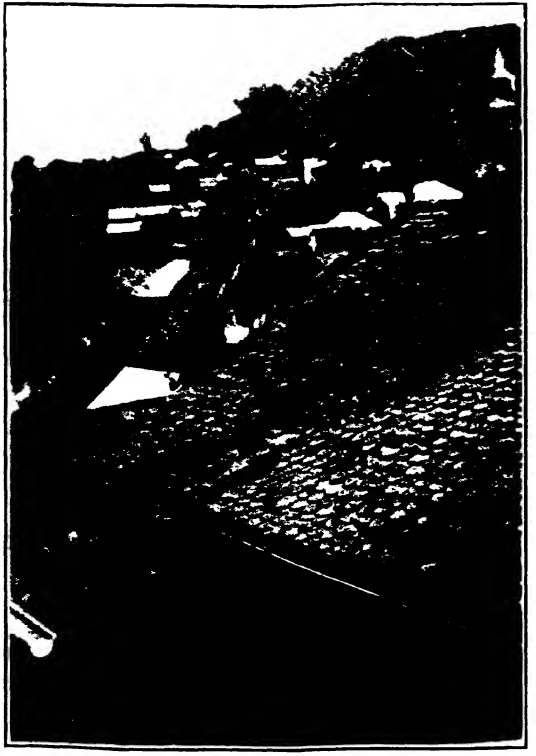
Swampy ground around contains Anopheline larvæ but the lagoon itself is harmless, and may be left as "a thing of beauty and haunt of game."

BOULDER STREAM IN CEROA (GRENADA).

The local Anopheline does not breed here unless in tracts fully exposed to the sun

BOULDER STREAM IN CEROA GRENADA)

The stream in flood should probably dispose of stray Anophelines.



IN A TROPICAL TOWN (ST. GEORGE, GRENADA).

A peep from a window—see cactus and various growth in gutter. Here the anomaly occurs of patients leaving the town for the country to recuperate from malaria!

GRENADA.

Moist Pasture, 800ft. elevation, on impervious rock, with abundant Anopheline larvæ practically the whole year round.

WAYSIDE DITCH (GRENADA)

Part concreted and only spasmodically tended Part beyond swarms with Anopheline larvæ

CONCRETING OF WAYSIDE GUTTERS

may create local safety against Anopheline breeding but too often the danger is merely handed on

SWAMPY GROUND (GRENADA)

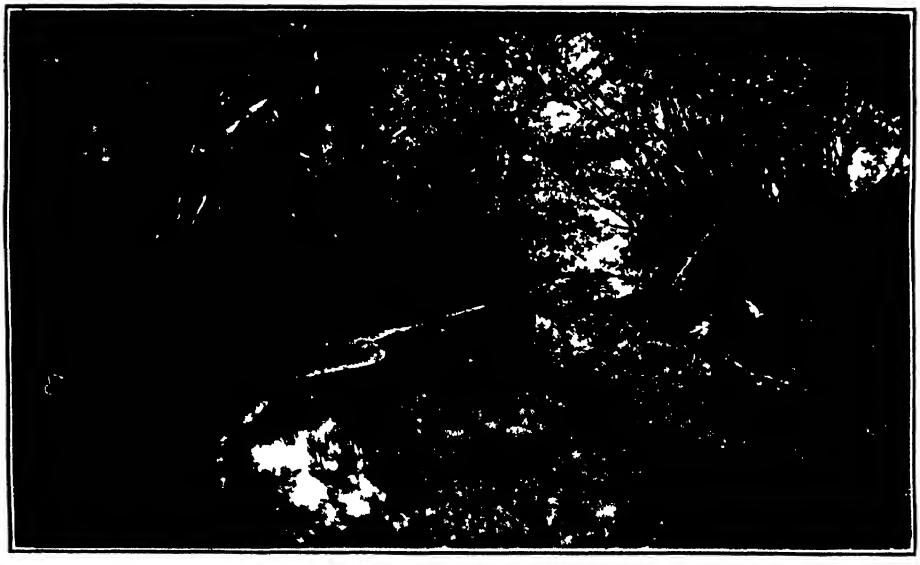
Shaded by Cocoanut Plantation No Anopheles to be found, although larvæ abound in the neighbouring wayside ditches exposed to the full glare of the sun

TROPICAL CITY (KINGSTON, JAMAICA 1911)

An odd lot of tin cans and receptacles galore collected on one empty lot in a city with a Yellow Fever record

BY THE SEA (GRENADA

Collecting *Stegomyia fasciata* larva from rockholes within one hundred yards of dwellings



SWAMP (GRENADA).

Culicines in crabholes No Anophelines

always to be found within one hundred yards of where the mosquito rests.

(b) Culicinæ generally.

General culicine limitation—leaving out for the moment the consideration of extensive swamps and lagoons and estates whose agricultural necessities create and maintain mosquito breeding conditions—is a matter of the prevention by the householder of the existence of permanent or temporary collections of stagnant water, and of the suitable treatment of unavoidable pools, temporary and permanent.

It is the duty of the sanitary officer to see that the people understand the possibilities and dangers of the conditions, and that they are conversant with and obey the statutory requirements.

The sanitary officer will also have the preventive control of all conditions of water accumulation on public property, roadside gutters, gullies, etc.

The sanitary officer and governments must remember to have their zeal tempered by common sense, and to keep ever before them in all matters of mosquito elimination the questions, What are the mosquitoes before us? and what diseases do they cause? A frequent illogical sequitur, since insect-borne disease has been studied, is: Certain mosquitoes cause disease; therefore, all mosquitoes must be exterminated.

Acting in this spirit, sanitary officers have called on their governments, and perhaps in more cases the governments, from reasons of personal zeal, have taken the initiative without special advice, with the result that costly schemes of reclamation of lagoon and swamp; costly purchase of dredgers and pumping tackle; costly inexpert administration have been devised to the tune of thousands of pounds, when a few grains of expert common sense and a few pounds in labourers' wages might have disposed of all nocuous possibilities.

The actual dealing of sanitary administration on large estates and in general where conditions of irrigation and even of swamp are necessary from the agricultural standpoint, is a question for local and individual decision and compromise; and in all—cacao, banana, rice, cane, jute, rubber, etc.—means may be come to of safeguarding the lives of the workers to the economic advantage, and not the loss of the employers of labour.

(c) Anopheles.

Speaking generally it may be said that permanent anopheline haunts

in relation to eligible populations are few; while temporary anopheline haunts in touch with the mass of a people in inhabited country are many

It follows that the first determination of the labours of the sanitary staff is toward the control of the temporary breeding places, which are many

This is a point long insisted on and insufficiently realised As a matter of fact its realisation is of little moment except in the possession of an organised sanitary department capable of acting on the realisation.

The control of malaria under widely varying conditions is one of the simplest of sanitary problems

The mere limitation of Anopheles records results of almost magical appearance notifications and deaths drop alarmingly ! (I speak of the result solely of anopheline limitation)

Measures directed alone to the disease and the infected have little effect on the extent of the incidence of malaria on any community.

The antimalarial measures usually employed may briefly be detailed —

1 Measures directed against Anopheles —
Insect trapping Practically useless as a preventive measure
Larva killing by larvacides, chemicals and oiling, fish stocking, etc ; regular sweeping of channels, double channeling of watercourses with weekly deviation and consequent desiccation of larvæ Of great value

2. Measures directed to prevent mosquito breeding —
Regular bushing and training of channels, roadside ditches, etc.;
Double channeling of watercourses with regular deviation;
Regular sweeping of all water channels, gullies, gutters, ditches, temporary pools,
Concreting of lengths of gullies, etc.,
Rubble filling of stormholes, etc ,
Grading of watercourses;
Canalisation, with regular sweeping;
Stocking of permanent pools, etc., with fish;
Draining of land;
Suitable cultivation to absorb swamp;
Saltwater flooding of swamps and lagoons;

Reclamation of swamps and lagoons by opening up, clearing and bushing, filling, draining, etc.;

Cultivation of shade trees along the course of streams;

These all are of value and refer to temporary and permanent breeding places alike; one, or many, or all may have to be undertaken at one and the same time; and the important point to assure is that the law puts them all under the administrative control of the health officer.

To discuss these various measures in detail is needless; the individual sanitary officer must control their relative usefulness in definitive situations.

I wish, however, to emphasise again the importance of persistent control of the temporary as opposed to the permanent breeding places of Anopheles. The former are in contact with the people; the latter are not so, or readily may be avoided. Swamp has become a malaria fetish to the unskilled; and the demand is frequent for thousands of pounds to be expended on reclaiming a wide lagoon, when adjacent pastures, cultivations, and roadsides are the real danger, and whose control is a mere matter of pence to the authorities and property owners concerned.

Some inexpensive bushing and clearing and drainage around will often remove all danger that was supposed to come from swamp and lagoon; and these might be left as things of beauty and haunts of game. Even the Anopheles might be retained a harmless creature for the delectation of the future entomologist. For, as Ross has well demonstrated, limitation alone will rid communities of malaria; and so soon as intelligent preventive measures are instituted and rigidly maintained as a departmental routine, communities far and wide should be kept malaria free without any fabulous expenditure, the dread of which has retarded sanitary administration in the tropics.

3. Measures directed to the disease and the infected:—

These may be summed up in Quinine, Screening, and the Mosquito Net.

The mosquito net is a domestic necessity for the European in the tropics, but for the people at large its use is a practical impossibility.

Screening has its usefulness in certain situations.

Quinine has its position in relief and cure, but in sanitary administration it has none.

Continuous drugging with quinine may prevent attacks of malaria in anopheline countries, but statistics are still lacking to demonstrate the extent to which the taking of this drug prevents infection.

That the human individual is detrimentally affected by the continued absorption of quinine, be he infected with malaria or not, there can be little doubt. Whatever are the causes of blackwater fever, there seems to be a consensus of opinion that quinine has much to do with it—perhaps more to do with it than malaria has.

STEPHENS and STOTT, in the March, 1915, number of the *Liverpool Annals of Tropical Medicine*, record a study of the correlation between blackwater fever and quinine, and suggest lines of investigation demanding accurate figures, which it seems almost impossible to obtain in numbers sufficient to be of statistical value. I would suggest enquiry into the gross relativity between the quinine consumed in any community and the incidence of blackwater fever.

In the West Indies, blackwater fever is comparatively rare. More of it has been recorded perhaps from the Canal Zone. In malarial communities in Africa it is common. There seems to be a distinct connection between wide quinine taking and blackwater fever.

Statistical evidence—in my opinion of equal value to that desired by STEPHENS and STOTT, and more readily obtainable—might be furnished under some such enquiry as this :—

Country :
Population—*A*, Native and Creole :
B, White and Foreign :
Estimate of Malaria Incidence on *A* and *B* :
Estimate of Blackwater Incidence on *A* and *B* :
Estimate of Quinine habit (prescribed and self-taken) on—
A, Malarial :
Non-Malarial :
B, Malarial :
Non-Malarial :
Actual Quinine Import to country :
Quinine Consumption per head of population :

It might further be ascertained in most cases of blackwater fever whether any quinine idiosyncrasy was known independent of the presence of malaria.

I have said that *quinine has no position in sanitary administration;* and I *contend that the facile relief from malaria attacks by the taking of*

quinine, the consequent apotheosis of quinine in the hands of the medical officer and in the minds of the people, the easy glory of cheap quinine administration by governments, have disastrously postponed sanitary administration in the tropics, and prolonged personal misery and economic inefficiency.

Large masses of peoples without taking quinine recover from malaria between each seasonal infection. Quinine relieves suffering, and by destroying parasites limits the number of infective gametocytes. I search in vain for figures to demonstrate the eradication of malaria by quinine administration alone; but I find readily that whenever preventive organisation has operated, malaria disappears "like magic."

VI. EDUCATION OF THE PEOPLE.

This obvious necessity is nowadays sufficiently recognised that mere mention is almost needless. At the same time the right method of popular education is hard to come at.

In the writer's opinion the chief work of the medical officer of health should be educative—personally educative in the homes and assemblies of the people. That a well-salaried officer should find the detail of a medical or sanitary appointment in the tropics to be beneath his personal recognition, otherwise than in directing from an office, is unfortunately too common, and is almost an established prestige of Colonial medical appointments.

Administration which should centre in the lay offices of the government (with medical advisors) is placed on the chief medical officer; and the energies of an otherwise "good man" are prostituted to controlling financial and structural detail, repairing door handles of hospitals, and seeing that the out-porter carries a box for threepence instead of sixpence, while of medical or sanitary knowledge he soon ceases to have a care, knowing that his salary and pension are irrevocably unalterable and assured.

Where a medical officer attempts to keep up to date in scientific attainment, and assert for himself a position in his profession as surgeon, therapeutist, sanitarian, he finds that the professional aspect of his position clashes with the lay administration, and he either sticks to his profession and is dubbed "bad administrator," or he accepts the

easier alternative of forsaking his profession and becomes a "good administrator."

Lectures are valuable stimulants in popular education, the subject and style being determined to the class of people.

The lantern is a powerful adjuvant; and the cinematograph, where obtainable and suitable films are secured, is of value.

Leaflets are useful for any type of population; but in the poorer and illiterate homes the medical officer and staff should make it their duty to read and talk over the subjects of the leaflets.

The domiciliary visitation of the sanitary staff should be educative all along; people desire to be clean and to be healthy. Dirt is mainly a matter of ignorance. The efforts of mothers for the good of their children are so easily to be elicited by the friendly education of the medical officer and sanitary inspectors. Any medical officer who has realised and acted on the belief that his best hygienic results are to be got from domestic education, must have had frequent pictures of remodelled homes to smile upon him.

In respect of malaria, the breeding haunts may not always be associated with domesticity; but much of the personal prevention of malaria infection is a matter of "clean living"; and to say that the home is the fountain of sanitary progress is as true of malaria as of typhus.

VII. ENFORCING THE LAW.

I have little to say about this; if there were more education in the land, statute books would be smaller.

Wilful wrongdoing is rare; the villain of melodrama is not often seen in real life. When we seem to have found him let us remember "*Tout comprendre c'est tout pardonner*," and realise that education rather than law is demanded; that in sanitary progress law too often spells stagnation, while education spells health.

To sum up the special references to malaria, I ask for the realisation that:—

The care of the health of tropical communities demands the general establishment of a Preventive Medicine Department apart from or part with existing medical departments, but, in any case, with specialised and experienced heads;

Malaria administration, to be of most advantage, should be part of the routine of any health department;

Malaria, in general, is acquired seasonally more from temporary than from permanent breeding places of anophelines; control of these temporary danger spots is a simple and inexpensive matter in the presence of the postulated health department;

Quinine administration for the relief and cure of malaria may be left to the medical practitioners, and should form no part of the armamentarium of the sanitary department.

In conclusion, I owe it to say that my opinions are founded on direct observation of malaria and malarial conditions in the West Indies and Central America; and certain animadversions may, to some, appear ridiculous when applied to other situations.

General study, however, of the literature of malaria and of the Reports of the Colonies leads me to believe that the main propositions hold universally, and that adaptation locally is the duty of the responsible officer.

Discussion.

Professor W. J. Simpson: I would like, first of all, to congratulate Dr. Angus Macdonald on his views—and robust views too—which he has expressed. He has very wisely limited himself to the conditions that he knows in the West Indies. He makes a surmise with regard to the medical reports of other colonies which he has read, and I may say that his surmise is correct. I am in entire agreement with him that malarial administration and the extermination of the mosquito should be in the hands of the Health Department as a matter of routine.

I assume that when he speaks about special commissions he is not referring to Scientific Commissions, but to Commissions which perhaps undertake work which can very well be left to the Health Department. Since the monumental discovery made by Sir Ronald Ross that malaria was a mosquito-borne disease, it then became the duty, I think, of the Health Department to apply that knowledge to the prevention of malaria, just in the same way as it deals with small-pox, cholera and typhoid fever. It is a matter of routine for the Health Department to remove the conditions that are favourable to

the causation of such diseases, and accordingly, in my opinion, it should be the duty of this Department to remove the breeding places of mosquito larvæ. It has been shewn by Dr. MACDONALD that these measures are not expensive, as a rule, if they are done in a systematic way. I quite agree with him that there is far too much inclination—in the popular mind—to fill up swamps and to undertake large works. Of course, there are certain swamps which must be filled up; but my experience agrees with that of Dr. MACDONALD that most of the malaria is due to the mosquitoes' breeding-places being close to dwellings. If the dwellings are quite close to the swamp, that is a different matter. There is also too great a tendency on the part of the Government to have water-tight compartments for different diseases and their prevention. I think it is a wrong policy. I believe that when special Commissions have thrown light on the causation of any particular disease and the methods by which it can be combated, the duty of dealing preventively with the disease should at once become the duty of the Health Department. But, at the same time, there must, with the increase of duty, be provided the staff for carrying out the extra work. As I have said before, I am assuming that Dr. MACDONALD'S references are not to Scientific Commissions. Nor, I should say, would they refer to the devising of new organisations for Health Departments; nor for consultation with sanitary experts, men who have perhaps devoted their lives to special subjects. I take it that the Health Officer in a far-off land is very glad to have the expert advice of a consultative scientific Sanitary Officer, in the same way that the practitioner at home is, at times, glad to have a consulting physician or surgeon to give his opinion on a case or subject.

Special commissions are also necessary for studying the cause of some new disease, and determining the methods to prevent it. But when these are known the application should certainly be held to belong to the Health Department.

With regard to other Commissions, it seems to me that there is a danger of overlapping, and a further danger that when the Commission goes home no further progress is made. I might give an illustration, such as the International Commission for the Prevention of Ankylostomiasis. I fear that if this does not fit in with the Health Department, as soon as that Commission goes away, and there is no provision for the Health Department to carry on its work, no further progress will

be made. Similarly, there have been a number of Malarial Commissions in India, which have concerned themselves mostly with indexing certain mosquitoes and their habits. But I hold the view that since Ross's discovery it would have been much more useful and more practical if methods had been devised for creating and organising a Health Service in villages and towns, and putting that discovery into practice, which can only be done by a proper Health Service.

I agree with Dr. MACDONALD that there can be very little progress without health laws. These health laws are the embodiment of the practical knowledge that we have gained concerning the improvement of the health condition of communities, and it is most advisable to have these health laws. But I am a little sceptical of the view which he expresses that health laws can be effectually put into operation by persuasion. He speaks about mothers being anxious to carry out the cleanliness of the domestic dwelling, and so on; but there are very many cities, especially in the East, where there are but few such mothers to be found, there being in those parts a large migratory population, and there the only "persuasion" is the enforcement of the law, with heavy penalties for failure. I believe under these circumstances that enforcement of the law is the very best teaching, even far better than lectures. It makes an appeal to the pockets of the individuals, and they soon learn that it is rather expensive to break these laws. There are many places that have no health laws and no health administration. Then there are places with health laws but no Health Department. There are also places with a Health Department but without good health laws; and there are places with a Health Department and health laws and yet the laws are not enforced. One has seen excellent health laws on the statute book which are not enforced until an epidemic occurs, and then only during the epidemic.

There is one point which, I think, has been omitted from the organisation which Dr. MACDONALD speaks of, namely, the Health Laboratory. I consider that any town of the dimensions he spoke of, or even a smaller town with a rural area around it, ought to have a Health Laboratory. It is there that important information is very often obtained about diseases. I remember that when I first went to India a laboratory was one of the first things I had established after the organisation of the Health Department had been completed in regard to the scavenging, the inspection and

so forth I found that the laboratory helped one immensely in dealing with cholera and other diseases

There was an interesting subject which I came across when I was in Dar es Salaam Professor KOCH had introduced a system there of giving quinine to the people in order to reduce the malaria I had a conversation with Dr ORENSTEIN, who was the Medical Officer of Health there at the time of my visit, and he had been Colonel GORGAS assistant at the Panama Canal He found that after a time the treatment of malaria by quinine was given up, not having been the success that Professor KOCH expected And Dr ORENSTLIN had come to the conclusion that the best methods for adoption were the elimination of the mosquito and the draining of certain swamps which were close to dwellings

I certainly think that we are very much indebted to Dr MACDONALD for his paper, and the conditions he has shewn us to exist in Trinidad

Mr T P BLDDOES There is no doubt that a great deal has been done in Trinidad, and one extremely appreciates the way in which the efforts have been detailed to us But one cannot help seeing that, not only there but in most of the West Indian Islands, and everywhere under the control of the Colonial Office, there is not that enterprise and that harmony of work that there should be

In the first place, there is the question—dealt with here and referred to in so many places—that the medical officer is simply a servant, to do just what he is told, and the officials sit down and say the Medical Officer will do what he is told And if he suggests that they should apply their minds to aid in determining the best way of getting rid of the anopheles, we find, possibly, that some man fills up a big pond or lagoon, which is of no use whatever Then it is said that so much has been spent on that that there is no more money available But the real fact is that there has been an unthinking amount of enterprise which has been limited simply by the amount of cash, it is not a question of the really useful work which has been done There is no doubt that is the tendency of human nature And, if anything, we see that more frequently now than at any other time We find even in the London daily papers comments to the effect that members of the profession are to do what they are told, or else the public will think they are ignoring the welfare of the children and the nation

When we come to the case of the small islands of the West Indies we see the need for officials to be energetic and to work in harmony; and particularly those abroad must have sympathetic support from the officials at home, and of that, up to now, there has been great need, and still greater lack. Individual efforts, such as those which have been put before us to-day, are of limited utility, because in one island one thing happens, and in another island another thing is done. There is no doubt that the real welfare of the tropics depends on health, and at no time in the history of the world was there such a shortage of food as there is now. Yet it is certain that food can be raised more economically, can be transported into the large towns more economically there, than in any other part of the world. There is sea transport, which is the cheapest form of transport; and instead of bringing it a long way over land, as sugar is often brought from Russia, and from another part which I need not mention, England particularly, and Europe generally, could have a much more plentiful food supply if efforts such as we have been told of to-day were really and properly organised, and received due help from London.

The CHAIRMAN (Sir DAVID BRUCE): We must thank Dr. ANGUS MACDONALD very heartily for his interesting and very useful paper.

He said in one part of his paper that "continuous drugging with quinine may prevent attacks of malaria in anopheline countries, but statistics are still lacking to demonstrate the extent to which the taking of this drug prevents infection." It is evident from that that Dr. MACDONALD is not a great believer in the efficacy of quinine as a prophylactic. I have often wished that this matter could be settled once for all by good evidence, but that has not, as far as I am aware, been done. I remember taking a party of natives, twenty years ago, into a very malarious part of Zululand. At that time the agency of the mosquito was still in the position of an hypothesis, but quinine was considered to be a protective against malaria. I therefore gave the natives ten grains a day, and took the same quantity myself. The tent was put up at the side of a swamp which was solid with mosquitoes, and in spite of the quinine we all got malaria. Ever since that time I have had a sceptical feeling in regard to the use of quinine as a prophylactic against this disease, and, as a result, I have never taken it as a preventive when in a malarious

country. Many white people when in malarious countries take five or more grains every day as a matter of routine, and continue the practice for years. If this could be definitely proved to be useless, it would save a deal of discomfort.

Dr. ANGUS MACDONARD: I thank you, sir, and you, ladies and gentlemen, for your appreciation of my paper, and I would thank Professor SIMPSON especially for his evident personal sympathy with my points of view.

As regards commissions, Professor SIMPSON is right in assuming that in my paper I did not stigmatise Scientific Commissions; what I spoke of was the type of local Commission which is composed of clergymen and ladies who have but little else to do. I referred to the sort of lay Commission which, in some colonies, is given the absolute administration of antimalarial measures, utterly regardless of the existence of any Sanitary Department whatever. And even our special commissions have their dangers, and, as Professor SIMPSON admitted, they are not always followed up with the practical work that the knowledge obtained, and demonstrated to the point of proof, demands. Even in reading the Reports of the Yellow Fever Commission one is disappointed at seeing whole volumes detailing investigations, when we must know that for many years, ten at least, the cause—whatever the ultimate particle of infection—the stegomyia, has been there. And alongside of these reports of the investigations in West Africa there is obviously an undercurrent running through it all suggesting that there is little or no practical work going on. The mere limitation of stegomyia seems to be absolutely neglected, as was evidenced in the same Report of the Commission. That emphasises what Professor SIMPSON said as to the Reports of these Commissions being acted upon too late.

With regard to the International Health Commission, that is a name for a body of American gentlemen who have been given the control of some millions of JOHN D. ROCKFELLER'S wealth to carry out one definite purpose, and that is to put the money into any part of the world, including those British Colonies which desire to accept it, and simply to ascertain who are infected with ankylostomiasis, to cure them, and to get as copious figures as to cures as they can. That offer has been accepted by our Colonies in many instances outside any Medical or Sanitary Depart-

ment. What the ultimate end or accomplishment of that Commission may be it is hard to see just now, because war events have complicated matters in the direction of this work having become an American affair. It has prevented our own men being set aside for that work, so that American medical men are at work in most of our Colonies, instead of, as in normal times, British medical men in British Colonies.

I realise and quite appreciate Professor SIMPSON'S point that my statement about persuasion in sanitary matters cannot be taken absolutely. There are situations in which circumstances may differ widely, and in some the firm hand of the law must be used.

I grant at once that my neglect to mention a Health Laboratory was an unfortunate omission, because that is an absolute essential. As for myself, when doing practical public health work in the West Indies, I made my own little laboratory, and in that way did what I could; and in Jamaica, at any rate, I had the help of the Government Laboratory for anything I could not undertake myself.

I cannot say much concerning Mr. BEDDOES' remarks, except that I believe he has a sympathetic feeling as to the difficulties which men anxious to do work in the West Indies come up against. That practically touches on the methods of administration, which have not, in my opinion, kept pace with the times, but which present events may quickly modify.

Sir DAVID BRUCE cheers me by his direct evidence of the non-prophylactic effect of quinine. Possibly he may in time become so sceptical about it as to be entirely with me.

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THE DEVELOPMENT OF PATHOGENIC PROPERTIES IN PROTOZOA, WITH SPECIAL REFERENCE TO THE HERPETOMONAD GROUP.

BY H. BAYON, M.D.

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Recent advances in our knowledge of the developmental cycles and incidence of certain protozoa which cause disease in man and vertebrates, enable us to speculate with some justification as to the laws which govern and the factors which induce the appearance of pathogenic and lethal properties in unicellular micro-organisms.

It may not be out of place to remark that the existence of protozoa harmful to man and the bigger mammals has focussed attention on the whole phylum, and stimulated investigation and research with very satisfactory results. We are, therefore, not only justified in considering

and studying the developmental morphology of any single group of protozoa, but also in observing the general biological rules which can be deduced from the behaviour of various protozoa under different circumstances, and comparing them with the conduct of their affinities.

Such discussions have been described as irrelevant, futile and unprofitable. They certainly do not always deserve this severe stricture. Anyone interested in research in the tropics knows the feeling of insecurity when observing apparently unknown or exceptional facts, which results from the knowledge that it is not possible to submit the result of one's fleeting observations to the crucial test of expert criticism—the surest and best incentive to research. Progress follows both inductive and deductive methods; it is not advisable to use either alone to the exclusion of the other.

It is quite comprehensible that the subject-matter of this paper is closely linked up with the question of parasitism, because though "parasitis" does not necessarily mean harmful or "pathogenic," yet it is evident that pathogenic properties cannot be evolved or become noticeable without a more or less prolonged stage of parasitism.

Parasites have been often classified and tabulated, and the terms employed for the purpose are generally known, *e.g.*, permanent parasite temporary parasite, obligatory parasite, facultative parasite, erratic parasite, etc. As a rule, only a small proportion of the terms employed in this connection are applicable to protozoa.

The views expressed in this paper are based on the behaviour of certain protozoa in their relation to man and other vertebrates. An attempt has here been made to classify various unicellular micro-organisms according to their biological features, whilst zoologists and protozoologists group the objects of their study according to some permanent or transitory morphological characteristic.

It is also evident that the term pathogenic applied to any protozoon designates the properties of some phase of its cycle in relation to some animal or group of animals; in fact, the word "pathogenic" describes the sum of the reactions between host and parasite.

I have ventured to tabulate protozoa of medical interest in the following fashion:—

Free-living or sapropelic. Ex., Infusoria.

Epizoic: all pathogenic, some even lethal. Ex., Ichthiophthirius, Leishmania.

Entozoic
- Saprozoic, usually enterozoic. Harmless, possibly even beneficial. Ex., Chlamidophrys.
- Pathogenic
  - Lethal. Ex., *Trypanosoma rhodesiense.*
  - Non-lethal. Ex., *Leishmania tropica.*

By considering the various fashions in which they damage their hosts we may divide protozoal parasites into those which destroy or necrose, such as *Entamœba histolytica,* or are proliferative, such as Rhinosporidium; or others which extract and possibly convert to their own use some necessary body substance, such as is done by *Plasmodium malariæ* with hæmoglobin. Or the damage may be purely mechanical, as we see in Sarcocystis. The lethal properties of certain trypanosomes do not appear to be due to toxines which can be demonstrated in the usual fashion; yet a "diffusive" pathogenic action is to be expected from the group.

If, again, we consider the localisation of the protozoa in their principal (vertebrate) host we see that the following variations of parasitism have been observed :—

Intranuclear = *Cyclospora caryolytica.*

Intracellular = *Plasmodium malariæ.*

Plasma parasites = Trypanosomes.

Intestinal = Giardia (Lamblia).

Intercellular = Sarcocystis.

In addition, I should like to make a distinction between a host that harbours a harmless parasite and one that is preyed upon by a pathogenic protozoon; the former is simply a host, the latter a victim.

The nomenclature having been settled, we can now proceed to consider and compare the properties of various pathogenic protozoa, including spirochætes and their relationship to free-living and saprozoic forms.

It is true that spirochætes are not protozoa, and DOBELL has ably pleaded their classification among bacteria. It cannot, however, be denied that at times they behave like protozoa, or, to sum up the situation according to FANTHAM, "that spirochætes are intermediate in character between bacteria and protozoa, shewing morphological affinities with bacteria, and physiological and therapeutical (biological) affinities to

protozoa." The following are the reasons which seem to place them in a group of "Proflagellata," intermediate between Protophyta and Protozoa:—

Bacterial characteristics.	*Protozoal features.*
1. Morphology very similar to that of certain known bacteria.	1. Longitudinal division.
2. Absence of definite nucleus.	2. Difficulty of artificial culture.
3. Transverse division.	3. Terminal, single flagellum.
4. Granules similar to spores.	4. Transmission by arthropods.
	5. Influenced by arsenical drugs.
	6. Movements follow a certain type of progression, whilst bacteria career aimlessly across the field of vision.

The argument of immunity is not decisive on either side, because agglutinating sera can be demonstrated against spirochætes, which though clumping in a higher dilution than those which are known against trypanosomes, yet are feebler than those which can be got by the injection of true spirilla.

In reviewing the principal features and properties of protozoa capable of causing disease in man and animals we note the following points:—

TREPONEMATA.

1. Free-living: saprophytic and pathogenic races.

2. Several pathogenic varieties. Habitat: plasma, inter- and intracellular.

3. Close resemblance between saprophytic and pathogenic races.

4. Hosts: numerous vertebrates. Victims: birds, mammals.

5. Transmission to victims: arthropods, especially ticks and lice. Also, by direct contact, *T. pallida* and *T. pertenue*.

HERPETOMONADS.

1. Free-living: saprozoic and pathogenic races.

2. Several pathogenic varieties. Habitat: epizoic and entozoic, intracellular.

3. Close resemblance between all varieties.

4. Hosts: arthropods, reptiles. Victims: mammals.

5. Transmission = ?

TRYPANOSOMES.

1. No free-living species, but varying degrees of pathogenicity.
2. Numerous more or less pathogenic species. Habitat: plasma.
3. Lethal species usually. Morphologically distinguishable from non-pathogenic.
4. Hosts (?) and victims: all classes of vertebrates.
5. Transmission: Hirudinæ, blood-sucking arthropods, or direct contact *(T. equiperdum)*.

AMŒBÆ.

1. Free-living: saprozoic (enterozoic) and pathogenic races.
2. Pathogenic varieties not numerous. Habitat: enterozoic and intercellular.
3. Pathogenic (lethal) species, similar to but distinguishable from simple commensals.
4. Hosts: practically every animal with a digestive tract. Victim: man.
5. Transmission: ingestive.

PLASMODIDIÆ.

1. Free-living: unknown. Affinities with Coccidia.
2. Numerous pathogenic varieties. Habitat: Erythrocytes.
3. Common features between all pathogenic stages; morphological differences between stages seen in birds and mammals.
4. Hosts: arthropods. Victims: birds, reptiles, mammals.
5. Transmission: arthropods. (Blood-sucking diptera in birds and mammals).

BABESIA.

1. All pathogenic, some lethal.
2. Numerous varieties. Habitat: Erythrocytes.
3. Considerable morphological resemblance between various species.
4. Victims: mammals.
5. Transmission: arthropods (ticks).

The purpose of this necessarily incomplete classification is to shew that the parasitic protozoa which have the most marked pathogenic properties for man and mammals are those which, as a rule, can be definitely distinguished morphologically from their saprozoic and harmless

relations. In other words, that a complete adaptation to a parasitic mode of life, with its corresponding alterations in morphology, results in an attendant enhancement of virulence. We must exclude from this rule the Herpetomonad group. The pathogenic, even lethal properties of *Leishmania donovani*, and the extreme similarity of its herpetomonad stage to saprozoic races, are a definite and glaring exception to this rule, which requires separate attention and explanation.

What does the increase in virulence consist of? It does not appear to be possible to frame any law which will adapt itself to all cases. We have already seen that the intensity of pathogenic action of single, even related protozoa varies within wide boundaries. One feature, however, appears common to most pathogenic protozoa—the facility and rapidity with which they multiply in their victim's body after a more or less prolonged period of incubation has been overcome. This might be explained by the necessity of maintaining the species in face of the evident disadvantage of this protective measure, which hastens the dissolution of the victim and menaces the parasite with extinction. It is an evident paradox that most lethal protozoa appear to be bound to their victims and an intermediate host; they are incapable of a free life-cycle, and, therefore, must at all costs gain entrance into their intermediate host before their victim dies, or else meet the same fate.

It is quite evident that a lethally pathogenic parasitism is not in the interests of the individual victim, and also not in the interest of the parasite. Protective measures exist to a certain extent in single instances, such as encystment in amœbæ and the presence of "reservoirs" in connection with pathogenic trypanosomes, such as *T. rhodesiense* and *T. gambiense*.

As to the trypanosome group we can see that the most virulent and pathogenic flagellates, though they are easily transmitted by inoculation from one animal to another, yet can only be cultivated with extreme difficulty if at all; some of the cultivations which have been published are merely conservation of trypanosomes in a suitable medium. On the other hand certain slightly pathogenic trypanosomes, of which we may take the *T. lewisi* as an example, are easily cultivable in artificial media, where they transform into crithidial and herpetomonad shapes.

Comparing these observations together, the conclusion can be drawn

that in the case of the trypanosomes of great virulence, such as those which have been mentioned, also *T. evansi*, *brucei*, etc., the adaptation to a certain form of parasitic life has become so permanent that a reversion to simpler forms is no longer possible. On the other hand, *T. lewisi*, with all its mild pathogenic properties adapts itself easily to varying conditions, and in so doing repeats its ontogenetical development in a somewhat similar fashion to what has been observed in the embryonal stages of vertebrate life.

These views, however, break down completely when dealing with herpetomonads, for though the leishmania stage is nothing more than an encystment, which can be easily brought about by injecting the flagellate in artificial culture into the peritoneum of a rat or mouse. It is true that this encysted stage can multiply without returning to its flagellate form, but still it can be turned into a flagellate with the greatest ease by placing some of the virus in citrated saline. Therefore we are face to face with a protozoon which is distinctly pathogenic, and yet has not adapted itself in a permanent fashion to parasitic life, because its morphological features are capable of reverting to those of free-living and saprozoic types.

In this connection it is well to consider the experiments of FANTHAM and PORTER, which shed much light on the subject. The conclusions of these two workers deserve to be quoted *in extenso* :—

> . . . . . researches have been conducted on the introduction into vertebrates of flagellates normally parasitic in insects. The vertebrates became infected by inoculation with the flagellates or by being fed on the insects containing the protozoa. . . . . Flagellates from sanguivorous and non-sanguivorous insects were used, and cold-blooded as well as warm-blooded vertebrates as hosts. The introduced protozoa were pathogenic to the mammals, but not markedly so to the cold-blooded vertebrates. *Herpetomonas jaculum*, *H. stratiomyiæ*, *H. pediculi*, and *Crithidia gerridis* (parasitic in certain water-bugs) proved pathogenic to mice. A puppy was infected by way of the digestive tract with *H. ctenocephali*. Frogs became infected with *H jaculum*, lizards with *C. gerridis*, and sticklebacks with *H. jaculum*. Second and third passages of some of the parasites were obtained. The protozoa, whether *Herpetomonas* or *Crithidia*, were present in the vertebrate hosts in either the non-flagellate or the flagellate form, or

usually both. They were most abundant in the internal organ of the hosts, more particularly in the liver, spleen, and bone-marrow.

It is inferred that the various leishmaniases are due to a herpetomonad of invertebrates which, under different conditions of environment, produce pathogenic effects in very varying degrees in different vertebrates, from zero, as in the mice described by DUTTON and TODD in 1903, to high mortality, as in Indian kala-azar, and probably zero again in cold-blooded hosts. It is also a flagellate which can probably live in invertebrates not already recorded as being infected. A human reservoir of leishmaniasis may occur in some places, while warm- and cold-blooded vertebrates may also function as the same. (FANTHAM, l.c. page 346).

Some years ago I described a herpetomonad found in the cloaca of *Chamæleon pumilus* at Robben Island. Its description has been published in 1913, and at that time I remarked that its principal interest consisted in the fact that it appeared to be a transitional stage in the development of herpetomonads from saprozoic protozoa in the gut of insects to permanent parasites in the cloaca of a vertebrate. Since then further specimens have been sent me by my late assistant at Robben Island, Mr. J. CAMPBELL, so that the occurrence of this interesting protozoon in the cloaca of the South African Dwarf Chameleon does not appear to be a rarity.

If we now consider the results of FANTHAM and PORTER'S experiments and the appearance of the herpetomonads found as saprozoits in reptiles, in the light of the experience that a pathogenic protozoon in a vertebrate is usually morphologically distinguishable from its harmless relations, we may be allowed to venture the suggestion that the anomalous position taken up by the Leishmania group is either due to the fact that their pathogenic parasitic and disease-producing rôle is a recent acquisition; in fact, if we consider that notwithstanding diligent search up to the present, no acknowledged intermediate arthropod has been found for oriental sore or kala-azar, we may come to the conclusion that it is quite possible that this group of diseases is acquired by the inoculation through blood-sucking insects of apparently free-living or saprozoic varieties.

The deductions of FANTHAM and PORTER'S work would therefore lead to a logical conclusion.

Reverting now to the behaviour of pathogenic treponemata, we see that according to the views expressed above, it appears that the striking morphological resemblance, amounting to morphological identity, which is found between *Spirochæta dentium* and *Treponema pallida*, which otherwise differ so greatly in their harmful properties, is certainly more like to what is observed among bacteria than protozoa. Among bacteria we have at times the greatest difficulty in distinguishing virulent from non-virulent strains; among protozoa, with the possible exception of certain infusorians of the Balantidium class, we have no such unsurmountable difficulty, especially if the anomalous behaviour of herpetomonads can be explained.

Arising out of these suggestions, renewed attention can be paid to the question of the origin of disease-producing protozoa. It is difficult at the present moment to bring forward any strikingly new views on the subject; still, a plea can be made for a broader view of the problem. A single explanation will hardly suit all instances which differ in so many particulars. In the case of *Entamœba histolytica*, simple repeated ingestion of cysts will give a satisfactory answer. For the Leishmania group the contamination of the wound, produced by a blood-sucking arthropod, with the contents of its gut, may possibly be applicable as a theory regarding the first steps towards parasitism in vertebrates. Pathogenic trypanosomes are possibly definitely specialised descendants of the harmless variety, which again are related to herpetomonads. The big group of Plasmodia present, however, quite a different riddle for solution. They are evidently permanent parasites of arthropods, or at least they pass the greatest and most important part of their life-cycle in arthropods, as real intercellular parasites, not simple inhabitants of the intestine. The adaptation to parasitism in vertebrates must therefore have taken place through regurgitation or direct inoculation through an insect sting. Three alternatives are then at least available to explain the initial acquisition of parasitism and disease-producing properties in protozoa. In course of adaptation to a parasitic mode of life other and new peculiarities were acquired, in addition to parasitism, as a result of the facility in obtaining nourishment from the host or victim; in addition

the higher temperature found in warm-blooded animals would encourage rapid division and multiplication, and therefore tend to perpetrate all the more any acquired morphological peculiarities or distinctive features.

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DISCUSSION.

Sir PATRICK MANSON (answering the invitation of the Chairman): I have no great desire to open the discussion on this paper, because the subject is far too complicated: it requires considerable thought before one could hope to evolve anything like a hypothesis on these matters. I think a hypothesis is a good thing, and, of course, it may be either right or wrong. And Dr. BAYON'S hypothesis may prove to be correct. Meanwhile, I think, it is better to suspend judgment. Perhaps someone more competent to open the discussion will do so.

A NOTE ON THE EGGS OF THE LIVER FLUKE, *CLONORCHIS SINENSIS*, VAR. MINOR (VERDUN AND BRUYANT), 1908.

BY

C. H. TREADGOLD, M.D., CAPT. R.A.M.C.

Recently, while examining fæcal material for amœbic cysts, I came across the eggs of this parasite in an Australian. The patient had visited different parts of China, and had also lived in places in Australia where the vegetables were exclusively supplied by the Chinese. The question as to which country was responsible for his infection is interesting in view of the fact that this trematode is held to be the cause of a serious disease of the liver which may terminate fatally; and in view of the fact that, up to the present, its eggs do not seem to have been reported in Australians who have never left their country. Unfortunately, our knowledge of the life history of these parasites is very limited. The only experimental work I can find on the subject was done by KOBAYASHI,[1] who claimed to have infected cats, rabbits and guinea-pigs with the cysts contained in the uncooked muscles of different fresh-water fish. Still, other sources of infection may exist, and in the absence of proof to the contrary we must admit the possibility of encysted cercariæ being ingested with imperfectly washed salad, grown in water contaminated by the excreta of infected orientals.

The Morphology of the Eggs.—The smallest seen measured 20·8 by 13μ, the largest, 29 by 16μ. The lid of the shell was rather flat, its margin being but slightly indicated; in fact, the operculum was only definitely made out in the minority of cases. The constriction described as existing

towards the anterior end was seldom well defined, but a small projection was occasionally noticed at the posterior extremity. It will be seen that the eggs differed appreciably from one another, both in size and appearance, and the question at once arises as to whether they all belonged to the same species. To decide this question a brief reference to the literature of the subject is necessary.

In 1883, BAELZ[2] gave a description of two new trematodes—common enough in Japan—*D. endemicum* and *D. innocuum*, the former alone being pathogenic. He described the eggs as varying between 20 and 30μ in length by 15 and 17μ in breadth, with a lid at the narrowest pole and frequently a projection at the other end.

In 1886, BLANCHARD[3] grouped the two forms described by BAELZ under the heading *D. japonicum*, regarding the differences as being too slight to justify the creation of separate species. BLANCHARD[4] states that the eggs vary between 23 and 30μ in length by 13 to 16μ in breadth, but that earlier observers give from 20 to 36μ in length by 15 to 20μ in breadth.

In 1907, LOOSS[5] described these parasites under the names *Clonorchis endemicus* and *Clonorchis sinensis*, the former being pathogenic and met with chiefly in Japan, the latter feebly or non-pathogenic and found principally in China. According to this observer the eggs of *C. endemicus* vary from 26 to 30μ in length by 13 to 16μ in breadth, while those of *C. sinensis*, although they do not differ markedly in length, are slightly wider (15 to 17μ). The narrowing towards the anterior end in *C. endemicus* is, in the main, not so marked, and the margin of the rather flat lid is not so sharply projecting as in *C. sinensis*; but these differences are, on the whole, very slight and not recognisable in every specimen.

VERDUN and BRUYANT[6] state that both styles of egg, as described by LOOSS, may occur in the same specimen. They regard the validity of the species *C. endemicus* as being insufficiently established, and think it more logical to create two varieties, *C. sinensis major* and *C. sinensis minor*, the former found chiefly in China, the latter—the pathogenic one—chiefly in Japan, Tonkin and Annam.

Conclusions.—The eggs found in this Australian patient correspond

approximately with the description given by Looss for *C. endemicus* (*C. sinensis minor* of VERDUN and BRUYANT).

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DISCUSSION.

Dr. R. T. LEIPER: I do not know that there is much that I can add to this interesting paper. Attention may be drawn perhaps to a recent publication, in which KOBAYASHI shews that the measurements of the eggs given by LOOSS as characteristic of the different species of clonorchis are found in different specimens of a batch of worms reared experimentally by infecting cats from certain freshwater fish. He also points out that the only specific difference given by LOOSS—the presence of a certain amount of pigmentation in *C. sinensis* in contrast to that of *C. endemicus*—is not a true specific difference, but occurs also in specimens reared in his experiments. The third point on which LOOSS differentiated the two species was the size. He has made out that *C. sinensis* was a much larger parasite than *C. endemicus*. But I think KOBAYASHI has pointed out that this also is incorrect. It should be noted, however, that when LOOSS wrote his paper he did not know that freshwater fish were transmitters of the disease. Now, a person eating an infected fish would probably take in several hundreds of the encysted cercariæ at one time, and probably these would grow fairly uniformly.

So that if a *post-mortem* examination were done some months later, most of the parasites would be found to be of the same size. This will perhaps explain why, in batches of material sent by collectors, there has been that remarkable uniformity of size, which has given rise to the notion that there are two species or varieties in *C. sinensis*. Of course, the fish is only the final intermediate host. Like all digenetic trematodes there is a mollusc involved as well. The essential larval metamorphosis is suspected to take place in a species of Melania. The terminal stage passes into the fish, which merely acts as a passive vehicle for the encysted cercariæ to the final host.

The author says, "We must admit the possibility of encysted cercariæ being ingested with imperfectly washed salad, grown in water contaminated by the excreta of infected orientals." I think we can only admit that if the encysted cercariæ have passed out of the fish into the water, and so contaminated the salad. That is a possibility, but, I think, a very remote one.

In the case of Paragonimus it has been found that the encysted cercariæ do migrate through the gills of the crab into water, and may be taken up by the definitive host from the water, but from what I have seen in other cases of encysted cercariæ in fish, a more probable explanation of the mode by which this Australian became infected is that he had some fish "sauce," or ate some uncooked fish during his visit to China.

THE HISTORY OF THE USE OF INTRAVENOUS INJECTIONS OF TARTAR EMETIC (*ANTIMONIUM TARTARATUM*) IN TROPICAL MEDICINE.

BY

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The history of any important discovery is always interesting, and that of giving antimony in large doses intravenously for different tropical diseases, is no exception to the rule. I hold no special claim to write this matter up, but having had occasion lately to consult the literature of the subject, before giving such injections in a case of ulcerating granuloma, I have thought that I might as well put the facts as I found them down, so as to save others the trouble and labour of wading through the references for themselves.

To NICOLLE and MENSIL[1] the honour is due for first proposing the use of antimony salts in tropical medicine, namely, in trypanosomiasis. Atoxyl and other arsenicals were then being largely used, both for experimental trypanosomiasis in animals and in human cases, and these authors not being entirely satisfied by the results obtained, suggested that antimony might be given a trial.

PLIMMER and THOMSON[2] followed this up and gave subcutaneous injections, of both the sodium and potassium tartarate of antimony, to experimental rats heavily infected with trypanosomes, and were at once struck with the wonderful sterilizing effect the drug had upon these parasites. Before treatment, a drop of blood examined microscopically, might be simply swarming with trypanosomes, but after one or two injections these had entirely disappeared.

Sir PATRICK MANSON[3] considered these results of such promise that he immediately gave two human cases, which he had under his care, similar subcutaneous injections, but unfortunately the local reaction of the drug, given in this way, was so severe that they could not be continued. He then tried giving it by the mouth.

About the same time BRODEN and RODHAIN[4] on the Congo tried giving the tartar emetic intravenously, natives suffering from sleeping sickness being treated in this manner. Naturally to begin with, as no one could know what exactly would happen, very small doses were employed, but as no abnormal effects followed, these were gradually increased until quite large amounts of the drug were taken. Clinically great improvement in the symptoms was noticed, and the difficulty of giving the antimony—a difficulty which had existed both by the oral and subcutaneous method of administration—was thus overcome.

LEBŒUF[5] claims that he independently discovered the intravenous route, and also gave injections by this channel about the same time as the authors just quoted. To these workers then the priority of giving antimony salts by intravenous injection belongs, and all subsequent work, both in trypanosomiasis, ulcerating granuloma and leishmaniasis, is based on their very valuable discovery. Some time after this MARTIN and DARRÉ[6] tried a combined treatment of atoxyl subcutaneously and antimony intravenously in a series of white men suffering from trypanosomiasis; very good results were again obtained.

Still later, KERANDEL,[7] who unfortunately had acquired trypanosomiasis himself, published his own case. At first treated on atoxyl he did not improve, so he next tried antimony by the mouth, having heard of the cases Sir PATRICK MANSON had treated in this manner. The drug caused sickness and could not be tolerated, so nothing remained but to try the intravenous administration. MARTIN and DARRÉ carried this out at the hospital of the Pasteur Institute with complete success, a cure resulting. Since that date a combined treatment by atoxyl and intravenous antimony (tartar emetic) has been carried out on most of the human trypanosome cases that have passed through the London School of Tropical Medicine,[8] and such treatment has been the standard one for this disease for many years.

The first records of the use of tartar emetic injections in leishmaniasis were made at the Brazilian Society of Dermatology in 1913, when

MACHADO and VIANNA[9] shewed cases treated in this manner. The case shewn by MACHADO was a woman aged 60 years. The WASSERMANN reaction in this case was positive, and though no leishmania were found, nevertheless the diagnosis of this disease was made, and the patient was given intravenous injections of tartar emetic. A complete cure resulted. The case exhibited by VIANNA also shewed good results from the intravenous use of the drug.

In the same year (1913) ARAGAO and VIANNA[25] treated cases of ulcerating granuloma by the same method with excellent results. In one case they gave twenty-one injections as follows:—First day, 0·05 of a gramme; second, 0·08 of a gramme; third to thirteenth, 0·1 of a gramme; fourteenth to seventeenth, 0·08 of a gramme; eighteenth to twenty-first, 0·1 of a gramme; or a total of about 1·8 grammes in all. In another case, a woman, ten injections were given within a period of thirty days. Rapid healing of the lesions took place in both these cases, the results being permanent.

TERRA and RABELLO[26] confirmed this work, using the same method of treatment for their cases. Intravenous injections of a solution of tartar emetic, dissolved in physiological saline, were given, one every day at first and then one on alternate days; after a few injections the ulcers began to shew signs of healing and completely disappeared after the fifteenth injection. According to their experiences tartar emetic is a specific for this disease.

During 1914, further papers on the treatment of cutaneous leishmaniasis by intravenous injections of tartar emetic were published by DA SILVA[10] and CARINI,[11] in Brazil.

Turning now to the New World, it was apparent to all that such injections might be useful for leishmaniasis as seen in the Mediterranean Basin and India; and in the latter part of 1914, CASTELLANI[13] treated a case of kala azar in this way in Ceylon, while DI CRISTINA and CARONIA[12] similarly used the method for the form of leishmaniasis found in the Mediterranean area (infantile kala azar). About this time, or a little later, ROGERS,[14] MACKIE,[18] and MUIR,[16] began to systematically treat Indian cases of kala azar by tartar emetic injections, obtaining results much more successful than by previous treatments. ROGERS in his paper apparently claims to have independently discovered this line of treatment for kala azar, but if the facts just detailed are closely studied

such a claim cannot stand. WENYON, in a summary of ROGERS's paper (Trop. Dis. Bul., Vol. VI., p. 221), rightly states the position as follows:—

> "A Notice of GASPAR VIANNA'S Treatment of American Cutaneous Leishmaniasis by means of Tartar Emetic appeared in this Bulletin February 14th, 1914. It is to this observer that the credit of first employing the drug intravenously in leishmaniasis is due, and to DI CRISTINA and CARONIA for using it in kala azar with success, must be given the credit of priority, even though the possibility of employing the drug in kala azar had previously been in the minds of most people having any knowledge of the disease."

This is correct as regards the Mediterranean form of the disease, and if any priority is necessary for the application of a well-known method of treatment to another disease, this would, for the Indian form of kala azar, as far as publication is concerned, go to CASTELLANI.

The results obtained by antimony in the three diseases quoted in this paper are in many ways good, and far in advance of other drugs that have already been employed; but at the same time it is necessary to add a word of warning. Its use has not prevented a high mortality amongst the Rhodesian and Nyasaland cases of human trypanosomiasis, and it remains to be seen whether it will act as a complete specific for all cases of leishmaniasis. As regards ulcerating granuloma, many cases are cured, but there still seems to be a small minority, which, though benefitted by the administration, are not absolutely cured. A case recently published by Dr. NEWHAM and myself[20] did very well, a cure resulting; but now we have a similar case in which though large numbers of injections of antimony have been given, no definite cure has been brought about. Still, a certain degree of improvement has taken place, and by a combined therapy of antimony and X-rays, we hope the desired result will be obtained.

The possibility of the use of antimony intravenously in other diseases, both in the tropics and in temperate regions, should not be lost sight of, in view of the results so far obtained in the diseases in which it has been employed. It might, for example, be given a trial in malaria and in other affections, especially of a protozoal nature.

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This list of references does not of course claim to be a complete one. It aims, as far as possible, at giving the chief or salient points of the history.

NOTES AND COMMENTS.

QUININE AS A PROPHYLACTIC.

BY

CAPT. C. F. HARFORD, R.A.M.C.

To my great regret I was unable to be present at the meeting of the Society on October 20th, to hear Dr. MACDONALD'S paper. If I had been there I should certainly have demurred to his statement of the case with reference to quinine, as it appears to me to be certainly misleading.

Quinine prophylaxis has been debated on many occasions in the history of the Society, and there has been overwhelming evidence in its favour. To recite this over again would mean elaborate references to tropical literature, and I am away from all such literature.

The pity is that those who criticise do not appear to recognise the position taken up by the advocates of quinine prophylaxis. Let me, then, state the position which I believe would be generally conceded.

1. There is no suggestion in these days that quinine prophylaxis should take the place of anti-mosquito measures. On the contrary, many who believe strongly in the importance of the former have been among the strongest supporters of anti-mosquito work.

Students trained by me in the elements of tropical hygiene have done splendid work in the elimination of mosquito breeding-places in Central Africa and elsewhere, yet they are strongly in favour of the daily dose of quinine where necessary. I have also written frequently on anti-mosquito work.

Sir WILLIAM MACGREGOR was one of the chief pioneers in the education of the Nigerian natives as to the rôle of the mosquito in the spread of malaria, which Dr. MACDONALD very properly advocates, yet he believed most strongly in the proper use of quinine. It was largely due to his influence that Europeans at work in West Africa adopted, to a very great extent, quinine prophylaxis, with the best results.

2. I am not aware that anyone contends that quinine prevents infection. What is contended is that quinine is the only agent known

which is antagonistic to the development of malarial parasites existing in the blood, so that where an individual has been infected an attack of fever may be prevented, or if one attack has occurred subsequent attacks may be warded off.

I do not think that anyone would be surprised at the failure of quinine to ward off fever under the conditions referred to by Sir DAVID BRUCE.

3. As to the suggested evil effects of quinine taken in the usual prophylactic doses, I have enquired very extensively into these, and, except in the case of idiosyncrasy, which is found in the use of most drugs, I do not believe that quinine is in any way injurious.

I am exceedingly sorry that the subject of blackwater fever has been brought up again without a particle of new evidence.

I consider that the great reduction in the incidence of blackwater fever in West Africa is largely due to quinine prophylaxis; but here, again, the marshalling of the facts which could be adduced in support of this view would occupy much space, and need reference to material which is not available.

So much for the main points at issue with reference to quinine prophylaxis.

Dr. MACDONALD says: "Quinine relieves suffering, and by destroying parasites limits the number of infective gametocytes." This is valuable testimony; but it is a pity that the previous sentence seems to discourage the use of quinine even in treatment, for he writes: "Large masses of peoples without taking quinine recover from malaria between each seasonal infection." Of course they do, as do people from most diseases without use of the appropriate remedy, but what of the mortality and suffering involved?

Dr. MACDONALD will, I hope, forgive these criticisms. We are all ready to support him in advocating the supply of an adequate sanitary staff in every colony, and probably we should agree that in the ordinary sense quinine prophylaxis, as regards natives at least, should not be dealt with by the sanitary administration. For Europeans, however, particularly in Central Africa, East and West, with its abundance of malignant parasites, there is good cause for advocating the use of a daily dose of quinine of five grains, which has in the past produced most important results.

Obituary.

We regret to announce the death on September the 8th, 1916, of Dr. R. H. Von Ezdorf, Surgeon U.S.P.H. Service, U.S. Marine Hospital, Mobile, Alabama, a Fellow of the Society of Tropical Medicine and Hygiene. Dr. Von Ezdorf did much useful work in Tropical Medicine, especially as regards malaria in the United States. These results were usually published in the U.S. Public Health Reports, his most recent papers being "Anopheline Surveys"; "Methods of Conduct and Relation to Anti-malarial Work"; "Malaria in the United States: Its Prevalence and Geographical Distribution"; and "Demonstrations of Malaria Control," the latter appearing in March, 1916. Dr. Von Ezdorf died suddenly, and was working right up to the end. His death is a loss to Tropical Medicine, and will be universally regretted by all workers in this branch of the profession.

G. C. L.

TRANSACTIONS

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THE VOMITING SICKNESS OF JAMAICA.

BY

H. HAROLD SCOTT, M.D., M.R.C.P. (LOND.), D.P.H.

Government Bacteriologist, etc.

My endeavour this afternoon will be to place before you a few facts concerning one of the most interesting diseases occurring in warm climates. At present it affects only a small part of our tropical possessions. No record of it is to be found in the majority of books on tropical diseases; in one well-known work there are a few lines, mostly wrong. As regards any definite findings as to the nature and causation of the condition, the affection spoken of hitherto as the Vomiting Sickness of Jamaica must be looked upon as a new disease. I will attempt in the time allotted to me to put you in possession of certain facts, which it has been my good fortune to work out and, in some cases, discover.

First, a few general remarks on the so-called Vomiting Sickness of Jamaica. The disease has existed in that island for many years, particularly prevalent in the cooler months, November to March or April,

and in each year it has been responsible for a considerable number of deaths, and in some years has exacted a terrible toll among children.

The earliest records which I have been able to trace date from thirty years ago, when the non-committal but descriptive name of "the Vomiting Sickness" was given to a condition which was beginning to command respect owing to its mysterious nature, its sudden onset, and its high mortality rate (80 to 90 per cent.).

Tracing the history of the disease from that time (1886) to the present has been an interesting study, but it must be passed over to enable us to come to more important matters. Briefly, the period 1886 to 1915 may be summed up by saying that, during the season in which the disease was usually prevalent, any child that died after vomiting was diagnosed (by some practitioners) as having suffered from vomiting sickness, while other medical men, rightly scorning so indefinite a term, erred in signing up true vomiting sickness cases as having died from gastritis, enteritis, worms, malaria, cerebro-spinal meningitis, yellow fever, and so on; while yet a third group—fortunately a small one—on attending patients with some obscure condition terminating fatally, would sign the certificate by the delightfully safe but paradoxical diagnosis, "vomiting sickness without vomiting," or, if of a classical turn of mind and desirous of impressing the relatives, "vomiting sickness *sine vomitu*." The name was, in fact, as serviceable as charity in the multitude of sins which it sufficed to cover.

In 1906, letters were sent to the other West Indian Islands, asking whether any such or similar disease was met with there, and the replies all went to prove that the condition is practically limited to Jamaica.

So serious were the ravages of the disease that, in 1910, an expedition was sent out from England to investigate it, but without result, and again, in 1912, a second expedition was made, which, in turn, came back baffled.

This was the state of things then, a veritable chaos, when, in February, 1915, a severe outbreak occurred on the north side of the island, and eighteen deaths took place in a small district in two days, and I was sent down to investigate the matter on the spot.

I had the good fortune to see a considerable number of cases, some almost from start to finish, and to perform autopsies on all who died during my stay in the district. I visited the huts where cases had

occurred, and, by the kindness of the District Medical Officer, was taken to interview survivors and the relatives of those who had died. There will not be time to go into the details of the many cases seen there; a brief summary will be given later, but before doing so it will be advantageous to describe to you shortly the symptoms of the condition. The usual sequence of events is as follows:—

The patient—almost invariably a child—in apparently perfect health, suddenly complains of feeling ill, and occasionally of pain in the stomach. This is usually mere discomfort and not, I believe, actual pain, but, as the native tersely puts it, "him belly trouble him, doctor." He then vomits; perhaps only once, perhaps three or four times at short intervals. Recovery then seems to take place, and, if the attack occurs at night, the child drops off to sleep, apparently well again. Some three or four hours later (occasionally after a longer interval) the child wakes up, again complains of feeling sick, and begins once more to vomit, usually frothy mucus, and later watery fluid only, or it may be bile-stained. There is little if any accompanying effort, unless the stomach be empty, when troublesome retching may ensue. Within a very short time, often a matter of a few minutes only, convulsions make their appearance, coma rapidly supervenes, and terminates in death.

Recovery from the first attack of vomiting being to all appearances complete, a doctor is not usually summoned until the relapse occurs. The majority of patients, therefore, first come under observation during the convulsive or comatose stage. The temperature is usually normal or subnormal, rarely 100° or 101° F.; the pulse is of good volume, and rate is 90 to 100 per minute; respiration 26 to 30, and regular until towards the end, when the Cheyne-Stokes rhythm may be noticed. The pupils are equal, moderately dilated, and, if the coma is not too deep, react to light. There is no delirium, and, shortly before passing into the comatose stage, the child may remark that it feels very bad, but does not call attention to any particular symptom or complain of any localised pain. There is no rigidity in the true vomiting sickness cases (except, of course, during a convulsion) but a general limpness of muscles; movement, such as turning the patient over for examination purposes, or to obtain fluid by lumbar puncture, frequently leads to a repetition of the vomiting.

Such is the picture of the majority (80 to 90 per cent.) of such cases, for the mortality rate is very high. In the rare instances which recover,

the vomiting is practically the only symptom. I have never met with a recovery when once convulsions or coma has set in. Recovery when it occurs is very rapid. Within twenty-four hours or so, a child who had appeared to be seriously ill may be up and about, shewing nothing but a little pallor and debility, as after any severe bout of vomiting; while others in the family, who did not seem any worse at the time, have passed into a state of coma and died in a few hours.

I crave your attention while we deal with a view of the symptoms in a little more detail.

1. *Vomiting.*—This may be, and usually is, the first objective symptom. The nature of the material vomited is usually at first food or if a considerable interval has elapsed since the last meal (and this is uncommon), frothy mucus, then watery matter, later bile-stained. At times the vomiting may be replaced by troublesome retching. Next, as regards the times at which vomiting occurs. In a typical case, as you have already heard, vomiting takes place at the onset, and may be the very first symptom. It is usually accompanied by considerable effort and is repeated two or three times, at short intervals. This I have termed the "initial vomiting," and gives one distinctly the idea of an effort on the part of the stomach to rid itself of some noxious material.

After an interval of calm, during which there are practically no symptoms, there is a return of the vomiting, and now it is of a different character. It is, to a great extent, effortless, and may be unaccompanied, by nausea. This I have designated the "secondary vomiting," and it is in my opinion, cerebral in origin, owing to its character of being effortless and induced by movement, and to its being followed almost at once by other nervous symptoms, twitchings, convulsions, and coma.

Initial or secondary vomiting may be absent. Thus, as stated already, the former only is seen in cases which recover—in other words, the patients never reach the stage when the secondary cerebral symptoms appear. On the other hand, this "initial vomiting" may be suppressed in the very rapid and acute cases. There is an attack of vomiting which is so rapidly followed as to be almost accompanied by the convulsions and coma, the entire symptoms being cerebral, owing, as I interpret it, to rapid absorption of the toxin from an empty stomach. Death in these cases may occur in an hour, or even less.

More rarely, the secondary attack of vomiting is suppressed; the

patient may pass through the initial attack and appear to improve; then, after a considerable but varying interval, he is seized with convulsions, passes into the comatose stage and dies.

Lastly, vomiting may be absent, and the cerebral symptoms may be the first indications of anything wrong. For example, I have among my notes the following case:—A child, four years of age, was quite well when she ate her dinner at 1 p.m. Two hours later (note the interval, please), she felt out of sorts and went to lie down. An hour or so afterwards her mother went to waken her, but could only partially do so; twitching of limbs and slight convulsions came on, and the child lost consciousness altogether and remained comatose till death.

Such cases have been diagnosed somewhat paradoxically as "vomiting sickness without vomiting," and I am sure that they do occur, though with exceeding rarity. Such a diagnosis, however, could not be made unless, firstly, true vomiting sickness cases were occurring at the time; secondly, all other causes could be excluded; or thirdly, the post-mortem signs, especially the microscopical, were those of vomiting sickness.

2. *Convulsions.*—These may vary from merely slight twitching movements of the limbs to definite massive convulsions. They may be tonic muscular contractions lasting for a few seconds only, or more clonic and epileptoid in character. Looking over my notes in cases which recovered, slight twitching movements occurred in one patient only, a child of four years of age. We have all of us seen slight twitching movements in a child asleep, in ordinary health, or possibly with a little dyspeptic disorder. With reference to the vomiting sickness one may safely say that in no cases which recover are convulsions seen.

3. *Coma.*—In most of the cases which I have seen this has been deep; as a rule, there was absolute unconsciousness with absent conjunctival reflex. In some, at an earlier stage of the coma, there was general flexion, and some irritability was exhibited when attempts were made to rouse the patient; but this "cerebral irritation stage" was transitory and soon passed into one of deep coma.

A few remarks may be added on the questions of age, sex, and duration of illness.

Age.—The condition is, to a great extent, one of childhood; sucklings are not attacked. I have records of only two cases under the age of twelve months, and these were not breast-fed. Nearly half the cases

(44·84 per cent. of my series) occur in the first quinquennium, another 30 per cent. in the second, and 85 per cent. of cases are under the age of fifteen years. The mortality rate is high in all these periods; thus, of those under five years, 85·06 per cent. died; of those between five and ten years, 86·21 per cent. died; and 84·24 per cent. of those attacked under fifteen years succumbed.

Sex.—The affection shews no predilection for sex. Thus, in the first decade, 45 per cent. of those attacked were males and 55 per cent. females; and the death-rate was closely similar, 46 per cent. and 54 per cent.; while in the third quinquennial period, of 58 cases of which I have full notes, 30 were males and 28 females; of these, 50 terminated fatally, and this number was contributed to equally (25 each) by males and females.

Duration of illness.—In 140 instances I have been able to obtain reliable accounts of the duration of illness. The shortest recorded was in a female child of one year, death taking place in thirty-five minutes. The average duration of the total number works out at 12·72 hours. Sex has no influence on duration, for, although of those whose duration is given 82 were females and 58 males, the length of illness from the time of onset to death—including, when present, the period of calm— works out at 12·5 hours in the case of males and at 12·89 hours in females, a difference of only 23 minutes.

It will not be time wasted if I briefly recapitulate the symptoms by shortly describing four cases exhibiting the different types:—

1. A mild case: a girl, nine years of age, was given some "soup" from boiled ackees and bananas at noon. At 2 p.m. she complained of pain in the belly and vomited. This vomiting continued on and off for three hours. She was seen by a medical man, who gave her a mixture containing ether and ammonia. The vomiting ceased, and the child had quite recovered by the following evening.

2. A male, aged three years, in normal health when he was given an evening meal of the soup made from bananas, yams and ackees. Two hours later, without any complaint of pain, he vomited. He rapidly recovered from this, and appeared quite well on being put to bed an hour or so later, and slept well till just before dawn. He then, without any warning, suddenly vomited again, was shortly afterwards seized with convulsions, and coma supervened, which lasted till death at 11 a.m. The

total duration was 16 hours; there was a symptomless intermission of 8 to 10 hours, and death occurred in 5 hours after the onset of the secondary vomiting. Here we have an example of a case apparently quite mild at first, but nevertheless terminating fatally.

3. A girl of six years, after a similar meal, went to bed in her usual good health. Early in the morning, without any warning or previous complaint, she suddenly vomited, and did so three times in the course of an hour. During the day she stayed in the house and did not feel quite well, but took food. She seemed better in the evening, and slept well during the night. Early the following morning, without warning, again she started vomiting, frothy watery material. without any effort. A few minutes later she was attacked by convulsions and passed into a state of coma, dying at 2 p.m. This case resembles the last, but differs in the longer duration of illness, and in the fact that the interval was one of improvement, not total abatement of symptoms.

4. A girl of twelve years left home in good health for school, three miles away. At midday she had a meal containing ackees, and returned to school, where nothing amiss was noticed, until three hours later she started to vomit; this occurred four times. Feeling better she started for home, but during the walk she felt ill again and vomited at intervals, taking three hours to make the three-mile journey. Shortly after arriving she became drowsy, this drowsiness deepened to coma, and she died about midnight without recovering consciousness. Here we have an example of a case in which convulsions were absent.

The rare condition of "vomiting sickness without vomiting" has already been described.

Let us now pass on to the pathological findings. I will not weary you with a long description of these. To describe them in detail would occupy more than the whole of the time at my disposal this afternoon. I have brought a series of specimens, some of which you will find under the microscopes, and I shall be very pleased to point out any of the special features to those who desire to know more of this interesting subject. The changes set up are very widespread, and may be briefly summarised as follows:—

Hyperæmia of most of the organs, including the meninges, with œdema of the supporting tissues; there is a tendency to hæmorrhages evidenced by small extravasations, *e.g.*, in kidneys, adrenals, lymph

glands, spleen, lung ; the epithelium, particularly of the kidney tubules, the pancreas and liver, shews necrobiotic changes ; and, lastly, and most important, is a marked fatty change in many organs, notably the liver, kidneys, sometimes in the cells of the pancreas and heart muscle, and the large Betz-cells and others in the brain.

Full description of the morbid anatomy, macroscopical and microscopical, is given in my larger paper on this affection published in the *Annals of Tropical Medicine and Parasitology* earlier this year.

Of course, in a disease such as this, which becomes epidemic every year, bacteria have been suspected and carefully looked for, without result. In fact, two years ago, during the course of my investigations, I stated " in my opinion the disease has no bacteriology. The organisms which have been found in some of the patients (a small percentage only) I can see no reason for regarding as in any way causative." The absence of prodromata, of any true incubation period, the serious and extensive lesions, the negative results of attempts frequently repeated at finding or isolating any organisms, all made me incline to the opinion that the condition was not a bacterial infection, but a pure intoxication, and we shall see how the sequel bore out the surmise.

We will now return to the outbreak on the north side of the island, of which mention was made earlier in the afternoon, for this was the turning point of the whole investigation, and after that events marched rapidly.

I investigated 32 cases during my stay in the district. A few of them may be briefly mentioned in order to make clear to you the sequence of events and the direction which my investigations subsequently took.

1. A male child, aged ten years. At 5 p.m., February 17th, complained of pain in the stomach and began to vomit. He vomited at intervals till 1 a.m., 18th, when convulsions set in, rapidly succeeded by coma, which lasted till death at noon. This patient had been quite well till the first vomiting at 5 p.m. At 3 o'clock, two hours previously, he had partaken of a meal consisting of yam, ackees, and bananas, all boiled together. The ackees were mostly removed and eaten by the older members of the family, leaving the soup, or " pot-water " as it is called, for this child and the one following, who, in addition to taking some of the soup, was given a little of the solid also.

2. Female, thirteen years ; cousin to the last, and living in the same

house. Also quite well until about 5 p.m., when she began to vomit and continued to do so off and on till midnight. In the course of the following morning vomiting again returned, and the patient soon after lapsed into coma, and death took place during the succeeding night. While comatose this child was restless, but did not have any definite convulsions.

3. The mother of the former and aunt of the latter, aged twenty-six years. Shared in the same meal, but only ate the solid ingredients (and naturally some absorbed fluid). Vomiting started about the same time (5 p.m.) and continued during the night, but ceased in the early hours of the morning. Later on, feeling better, she took some more of the same articles of food. Vomiting set in again the same evening and persisted at intervals during the following day. There was no convulsion, no coma, and the patient recovered in 24 to 36 hours, though she felt weak.

Two other adults, aged twenty-nine and sixty-five respectively, of the same family, suffered similarly to the last, but recovered in the same way.

I would ask you to notice in this series that the first meal was shared in by all; the subsequent one contained the same ingredients. The adults ate the solid and recovered after pretty violent attacks of vomiting; the older child had some of the solid and some of the soup and died after 34 hours; the younger had soup only and died in half that time; and the interval between the meal and the onset of the vomiting was in each case about two hours.

One more series, this time of three. A girl of eight years, a boy of two years, and their mother, twenty-five years of age. All partook of a meal consisting of yam, banana, pumpkin and ackee boiled together. The other child had a little of the solid with some of the "pot-water." She suffered with vomiting, succeeded by convulsions and coma, and died in 16 hours. The younger had the soup only, suffered with the same symptoms but more acutely, and died in two hours. The mother ate the solid ingredients, was acutely ill with vomiting, so ill in fact that, as she expressed to me with tears in her eyes, she was unable to attend to her children who were dying. Her attack of vomiting continued for nearly 24 hours, after which she rapidly recovered.

If I may, without trespassing too greatly on your indulgence, I would like to quote one last case:—

A child, a little girl of three years of age, at 11 a.m., February 24th,

was given the liquid or "pot-water" made from yam, peas and ackee. At 1 p.m., two hours later, please notice, without any symptom or complaint during the interval, she vomited twice and went to lie down. At 2 p.m. the attack recurred, muscular twitchings and convulsive movements supervened, consciousness was lost and the child died comatose at 5 p.m., the total duration of illness being only four hours.

I will not weary you by further narration of cases. I have notes of between 300 and 400 now, and could quote several such series as those just detailed.

To sum up the 32 cases which came under my notice in this outbreak: in 17 the attack followed closely on the ingestion of ackees or a watery extract (soup or pot-water) made from them. In eight others there was a strong probability that ackees comprised one of the constituents of the meal prior to the onset of the illness. In these instances the parents had had a meal containing ackees and the children "may have had some." In the remaining seven cases no definite history of the food could be obtained; but I visited the huts in which the cases had occurred and noticed in every instance, without exception, that trees bearing ripe fruit were growing in the yards, and it is most unlikely, to say the least, that such a food, ready at their very doors, a food of which they all are fond, and which was then ripe would be avoided, especially at a time when other articles of food are scarce or at least relatively expensive. In none of the 32 then could the eating of the fruit be definitely excluded.

The next point was to make enquiries concerning the ackee, the fruit of *Blighia sapida*, which is used to a considerable extent as an article of diet in Jamaica. Amongst the better classes the ackees are gathered carefully, one by one, and only those which are properly opened and appear ripe and sound in every way are taken for food. Unopened ackees are not used by such people, nor any which have not opened naturally on the tree or have been gathered from an uninjured branch; those forced open after falling from the tree unopened are dangerous. Among the poorer people, however, less care is taken, and a boy is sent up the tree to shake down the fruit; ripe and opened and unripe unopened fall together; the former is collected and the latter left. In time, some of these may open and be gathered with fresh ripe ackees brought down at the next shaking.

By the time the investigations into the Montego Bay outbreak and the examination of the various tissues taken post-mortem were nearing completion, I considered that sufficient evidence had been presented to warrant the bringing in of a true bill against the ackee, sufficient, that is, to put it on its trial, so experimental work was started with this end in view.

Details of my earlier experiments and their disappointing barrenness of results have been given in the monograph already alluded to The reasons for their failure were disclosed subsequently, but it would be profitless now to relate either the experiments or the causes of their failure. I will merely deal briefly with those which arose directly out of the indications obtained from a study of the outbreak described above. The aim in view as you will have gathered, was to establish whether any, and if so what, connection existed between vomiting sickness and ackee poisoning.

In order to simulate as closely as possible the conditions under which, by this hypothesis, cases of vomiting sickness occur, some ackees were obtained which to all appearances were good except that they were unopened or had been forced open after being gathered. The part used for food was then boiled with water just as was done by the natives in making their soup or "pot-water." The product, practically a watery extract of ackee, was then filtered. The result is a liquid of the colour of weak tea with a layer of oily, fatty matter like melted butter floating on the surface.

This extract was administered by mouth to kittens, a pup, guinea-pigs, rabbits, Belgian hares. The three last-named were unaffected. The other animals exhibited the same train of symptoms, so that the recital of one will suffice for all. Within an hour of administration of a small quantity vomiting set in, and the animal was inclined to be heavy and dull for about half to one hour. Recovery then took place and the animal became normal and lively again. The following day a slightly larger dose was given with similar results, recovery being apparently complete in two hours or a little more. After an interval of three hours a third dose was given twice the size of the first. Vomiting came on 45 minutes later, and the animal became dull and drowsy, its head nodding as with sleep in some cases, in others it merely lay about and was disinclined to move and vomited at intervals. This drowsiness

gradually deepened to coma during the succeeding hour, and death took place some four hours after the last administration. The total amount given was the extract from one ackee. The post-mortem appearances were, both naked eye and microscopically, absolutely typical of those found in human vomiting sickness patients. I have the specimens here for you to see this afternoon. I repeated the experiments on several animals, merely varying the dosage, and except for a slight difference of interval between the feeding and the onset of the vomiting, according as the dose was small or a little larger, the symptoms and post-mortem appearances were the same.

Briefly stated, the characteristic symptoms of the so-called vomiting sickness appear an hour (more or less) after the administration of filtered watery extract of ackee. In human cases, where other food was taken as well and the action probably slower in consequence, the interval was usually two hours. After a small dose there was vomiting, and after a larger still, vomiting, drowsiness, coma and death.

The matter had by this time progressed beyond the realm of mere hypothesis, and a most welcome confirmation came three months later, when the following case occurred:—

On the evening of August 19th, 1915, a family of eight, all at the time in good health, partook of a meal of ackees taken from a branch of a tree which had been damaged by the hurricane of the previous week. About two hours later, five of them complained of feeling sick; later three of these were attacked by vomiting, and one who had drunk some of the soup, shortly afterwards became convulsed, rapidly lost consciousness, and died within 24 hours of the meal. The remainder completely recovered.

Six days afterwards, at 6 p.m., another similar meal was prepared. The soup together with some of the boiled ackees were eaten by a woman twenty-four years of age. At 8 p.m., she vomited and soon afterwards stated that she felt better; at 10 p.m., however, the vomiting returned, convulsions followed, coma set in and death took place shortly after midnight. Another member of the family was also taken ill, but recovered after vomiting. The autopsy I carried out myself and took specimens of practically every organ and tissue. Full details of both macroscopical and microscopical appearances have been given in the paper already spoken of, and I have brought sections with me to-day.

Here then was a definite history of a patient previously in good health partaking of a meal of ackees from a bruised limb. She, with other members of the family, suffered from vomiting and recovered. A week later another meal was prepared with fruit from the same tree. The patient drank the soup and also ate some of the solid. Two hours later the symptoms appeared and ran their course to fatal termination in six hours or so, and at the post-mortem the changes were revealed which have been mentioned earlier, and which you can see for yourselves afterwards. In this case the term "vomiting sickness" was not used from first to last, but the case shewed typically the onset, course, and pathological changes of that disease.

Certain peculiarities and characteristics of the affection which at the outset were most puzzling, find a ready explanation in the light of our present knowledge of the similarity (may one say identity?) between vomiting sickness on the one hand, and the effects and results of experimental administration of ackee extract on the other, linked together by the clinical case of definite ackee poisoning just related. These were:—

1. *The peculiar seasonal prevalence.*—The epidemic character of the disease corresponds exactly with the main ackee season, when other fruits and natural foods are relatively scarce. If the ackee season lasts longer than the usual November-December to March-April, then also cases of vomiting sickness continue to be reported for similarly longer periods. Ackees are also obtainable in smaller quantities at other times, but other foods are then plentiful and this fruit is less eaten. Occasional cases of vomiting sickness, however, appear at other times as the one just related. It used to be thought that it was a disease of which occasional, sporadic cases occurred during the warmer months, becoming epidemic in the cooler, comparable, for example, with cerebro-spinal fever due to the meningococcus.

2. *Limitation to Jamaica.*—The results of the circular-letter sent to the authorities of other West Indian islands have already been mentioned. I myself have made enquiries of inhabitants of other islands and am told that the *Blighia sapida* does not grow to any extent in any of them. It is true that one or two trees are found, for example, in St. Lucia and, I think, Barbados, but they are looked upon as curiosities and are not used for food.

3. *Sudden onset of symptoms* in the midst of apparent good health,

without any incubation period or prodromata, and in the well nourished and not necessarily the emaciated or debilitated. We see now that the symptoms, being those of an acute intoxication, would depend not so much on the general well-being of the subject as on the dose of the poison and the condition of the stomach, whether empty or full, and its consequent readiness for absorption.

4. *The rapid and complete recovery of non-fatal cases.*—This is obvious, and explained by the fact that an acute vegetable poison is taken; if the dose is small it is got rid of by the vomiting, and the patient recovers.

5. *Affection of several persons practically simultaneously in one house* or close neighbours in a settlement. Several members are affected in one house because the food is cooked together and shared in common. Close neighbours in a settlement are affected because the trees are in and about the settlement and all share in the produce.

6. *The vastly greater preponderance in children.*—This is explained by the fact that they are given the "pot-water," the most toxic part—an extracted poison, in short—and that the lethal dose of a poison is far smaller for a child than for an adult; and also the adults know the risks of eating unopened ackees while children naturally do not.

7. *Attacking the West Indian native in much greater numbers than the East Indian or the white man.*—In Jamaica the coolies live largely on rice and split peas, often in the form of curry; they also like green fruit—mangoes, guavas, jack-fruit. They rarely indeed eat ackees. A few, after they have served their time and settle in Jamaica, may eat them, but not at all commonly. The white buys his ackees in the market, where he can see and select them; while, safer still, many will only eat ackees which have been carefully gathered under their own superintendence and from their own trees.

Further investigation I hope to undertake later on to elucidate, firstly, the actual constitution and nature of the poisonous ingredient. Whatever its nature it appears to be rendered inert, partly if not completely, by alcohol. Patients seen in quite an early stage, the initial (gastric) vomiting period, had the best chance of recovering on the administration of stimulant—ether, rum, whisky, brandy. This is further borne out by the fact that extracts made with alcohol, ether, or petroleum

ether were, as far as I have been able to test them, innocuous, while the watery extract, as you have seen, proved rapidly fatal.

Secondly, to try to find an antidote, though the action is so rapid that one cannot hope much from it, even if discovered; and, thirdly, to try to explain why this substance should act so energetically on carnivora—human subjects, kittens, dogs—and have no effect on herbivora intragastrically. I have administered to guinea-pigs and rabbits several times the quantity of the same extract as sufficed to kill a kitten, but without any obvious untoward effect.

I may add that on the completion of the investigations which have been related, I was asked to draw up a notice of precaution as to the use of this fruit as a food. This was done shortly before my departure from Jamaica last year, and I have had a letter from the Governor, and also from others in the island, telling me that the cases of vomiting sickness last season were exceptionally few.

To sum up:

1. The term "vomiting sickness" has been used in Jamaica for many years as a comprehensive name for various diseases, including cerebro-spinal meningitis, gastritis, gastro-enteritis, worms, malaria; in fact, any disease occurring in the cooler months and associated with vomiting and convulsions.

2. During the last ten years the idea has been gaining ground that there is an affection included under the term "vomiting sickness," whose course of symptoms and post-mortem changes are not those of any known disease.

3. The death-rate from this affection is exceedingly high, 80 to 90 per cent., and a fatal termination takes place in a few hours.

4. Investigations into a typical and severe outbreak in February, 1915, revealed the fact that, in a majority of the cases in which a reliable history was obtainable, ackees formed part of the last meal taken in health, and that this article of food could not be excluded in a single case.

5. Persons drinking the soup or "pot-water" made with ackees in certain conditions shewed the most acute symptoms; the onset occurred in about two hours, and death nearly always resulted.

6. The fruit is poisonous if picked from a decayed, bruised or broken branch; if forced open and not opened naturally on the tree, amongst other conditions.

7. Much of the poison is extracted by boiling with water. .

8. The symptoms of a case of typical vomiting sickness are: initial vomiting (gastric in origin) coming on in apparently perfect health; a period of improvement lasting a few hours, succeeded by secondary vomiting (cerebral), rapidly followed by convulsions, coma and death. The average total duration of illness is twelve and a half hours. Initial or secondary vomiting or convulsions may be absent, but not in a large percentage.

9. Recovery, in my experience, has never occurred when once convulsions have set in, or coma if convulsions are absent; and as a corollary to this, in no cases which recover are convulsions seen.

10. The affection is largely one of childhood, and shews no predilection for sex.

11. A reasonable interpretation of the symptoms is: some poison is taken, or some substance which acts as a poison after it enters the stomach. If the initial vomiting is able to get rid of this substance no further symptoms occur, and recovery is rapid. If this is not the case, there is an interval, a more or less quiescent period of absorption, after which there follow symptoms due to the action of the poison on the higher centres—secondary (cerebral) vomiting, convulsions, drowsiness, coma and death.

12. In rare instances the cerebral symptoms are those first noticed—convulsions, drowsiness, coma; there is no preceding vomiting—the so-called "vomiting sickness without vomiting."

13. Intragastric administration of an extract made by boiling unopened ackees with water produced in certain laboratory animals (kitten, dog) the symptoms and pathological changes seen in cases of vomiting sickness.

14. A case of ackee poisoning in a human subject exhibited the same symptoms, course and post-mortem changes, macroscopical and microscopical, as *(a)* human vomiting sickness cases, and *(b)* animals to whom an aqueous extract of unopened ackees had been administered.

15. The characteristics of vomiting sickness, the seasonal prevalence, the sudden onset in health, the rapid and complete recovery of non-fatal cases, the rarity of occurrence in white children and East Indians, the pathological changes set up, and so on, all find explanation in the view that the condition is an acute intoxication by the unwholesome ackees—the fruit of *Blighia sapida*.

Dr. G. C. LOW: We must thank Dr. SCOTT very much indeed for coming and giving us this very interesting paper to-night. I am very sorry we have had such a poor audience to receive and listen to it, but, even though this is so, it will go out in the TRANSACTIONS, and I have no doubt that many members who have been unavoidably prevented from being here this evening will read it later with great interest and pleasure.

There are several points which, I think, Dr. SCOTT might follow out, or get followed out, further. For instance, why not have the fruit analysed, in order to ascertain what the poison in it is? It certainly seems very strange why the fruit from a bruised branch, or fruit which has been opened prematurely or unnaturally, should be poisonous, while the ripe fruit which has burst open of itself is not poisonous. It seems difficult to say exactly why this should be so, though, of course, it might be that the poison is a volatile substance—a substance which has escaped in the open ripe fruit, but not in the case of the immature fruit. I do not feel any doubt in my own mind, after hearing Dr. SCOTT'S paper, that the poison of this fruit is the cause of all the symptoms from which these patients suffered.

Another interesting point, which might also be gone into in detail, is whether this fruit grows in the other West Indian Islands as well as in Jamaica. I would ask whether Dr. COCKIN has seen ackees in Grenada. If the fruit grows in other islands in the West Indies, do the natives there eat it? Also do they eat it in other parts of the tropics, such as the West Coast of Africa?

The microscopic specimens we have seen to-day are certainly very interesting and instructive; they shew very toxic changes, the fatty degeneration and lesions in the liver and other viscera being extreme.

It is quite conceivable that these might be mistaken for yellow fever, and it is clear that ackee poisoning must be considered in the differential diagnosis of that disease.

With regard to guinea-pigs, rabbits and other animals being immune from symptoms when the immature fruit is given them, whereas it has strong effects upon kittens and dogs, the same sort of thing is seen with other plants even at home here in England. Rabbits and hares may eat different shrubs with immunity, whereas if these were given to carnivores poisonous results would quickly follow.

Dr. R. P. COCKIN: With reference to Dr. Low's remarks concerning Grenada, I am in a position to state that ackees do exist there. They are, however, regarded—as, I believe, is also the case in St. Lucia and Barbados—as botanical curiosities, and the fruit is not eaten.

I would like to ask Dr. SCOTT if, during his experimental work on animals, the solid part of the ackee fruit was administered with the oily extract, with the view of ascertaining whether the solid part is capable of fixing the "oil," and so having an antidotal action.

I would also ask him if he can explain the marked degree of fatty degeneration which his stained sections shew takes place in the liver and other organs. The extent of this fatty degeneration is out of all proportion to the short duration of the illness, and I should be grateful for information on this point.

The CHAIRMAN (Sir DAVID BRUCE): We must all heartily congratulate Dr. SCOTT on having brought this investigation to a successful termination. Many other men attempted to solve this problem, but all failed. It may be said that the solution is a simple one, and might have been solved by anyone who could look beyond his nose. But this simplicity is the characteristic of all great inventions and discoveries, and it would seem the power to look beyond their own noses is given to few. Dr. SCOTT evidently belongs to this small and select band, and we hope that he will have many more opportunities of using his gift for the advancement of knowledge and the increase of prosperity to mankind.

Dr. SCOTT: With regard to the analysis of the fruit, this recorded investigation was practically finished in August and September of last year. In the first investigation, I got the Deputy Island Chemist out there—who is an excellent man—to make the analysis. Thinking there was so much fat in it, he made it with alcohol, with ether, with petroleum ether, but so far as one could judge, such extracts were inert on the laboratory animals which were experimented with. And I think the mere fact of using these alcoholic things did not extract them. I said if you catch a child in the early stage of the condition, give it as much alcohol as it will take, as it is likely to precipitate the poison, and no secondary symptoms occur. I think in that way the chemist was

missing out the toxic principle. When making the soups with boiling water, the poison is practically extracted, although there is this fat on the top. It comes down even on filtering.

With regard to the question of the poison being a volatile substance, cassava is a case in point. The native will not take it until it has been washed three times in order to get rid of the prussic acid. The solid part was not administered with the liquid in these experiments, because I found that the most fatal cases were those of children who were given the soup or "pot-water." The older people, as a rule, after boiling, took out the solid part and ate that. Some of the adults who suffered severely did so, I believe, because they had some of the soup with the more solid portion. The older children who were given some solid and some soup were very ill indeed, and most of them died. The babies who had the soup alone invariably died. Therefore I cannot answer the question as to the solid being possibly an antidote to the extract.

The reason why the liver is so greatly affected I am afraid I cannot say. If the patient has died some time after the symptoms set in, all the organs—heart, pancreas, even the lungs—shew this fatty condition; the same is true of the lymph glands, as in the specimen I shew you. I think the poison, being in the stomach, is absorbed straight away through the vessels. It probably gets to the liver first, and thence passes into the general circulation. If you look at the cerebral sections you will see there are occasional drops of this staining with osmic acid in the smaller vessels, as well as, but less marked than, in the big nerve cells themselves. It looks as if the liver made an attempt to stop it—which agrees with the calm interval—and that it then got beyond that organ, and that it is when it reaches the brain that most of the symptoms are evident. I have found fat even in the heart muscle in these cases.

I cannot answer Dr. Low's question as to why the unopened fruit is toxic: that is the direction the further investigation will take. The natives know the fact themselves. I shewed them several fruits, and asked, "Would you eat this?" "No." "Why?" "I don't know." I think it may sometimes be due to it not being quite ripe. It is usually those with a small seed, the fruit itself not having properly developed. It is not hydrocyanic acid, because that would be given off in boiling. It is very difficult to get the history of a case from the native, as he fears being had up for poisoning his child: he would have to admit having

INTESTINAL PARASITES IN NORTHERN SIAM.

BY

DR. W. F. J. KERR, CHIENGMAI, SIAM.

During the past year, I carried out an investigation to determine the relative frequency of infection with various intestinal parasites among the prisoners in Chiengmai jail.

A series of 230 adult male prisoners was examined. Of this number 74 per cent. were Laos, and 13 per cent. Shans and Karens, all natives of Northern Siam, most of whom had spent their whole lives in the Chiengmai district. The remainder were made up of small numbers of Siamese, Yunnanese, Indians, and Burmese. The Chiengmai district was, until the last two years, two or three weeks' journey from the nearest seaport; now an advancing railroad brings it within five or six days of Bangkok.

The routine followed in every case of this series was to make a microscopical examination of the motions, and then to give an anthelmintic.

For the microscopical examination very thin films of fæces were employed, always diluted with water, unless the motions were liquid; 21/26 mm. coverglasses were used, and the whole of each film gone through. In about 70 of the earlier cases a second film was only examined when no hookworm ova were found in the first; in all the other cases two films were examined. A laxative was given before the microscopical examination to ensure getting a motion, but the motion so obtained was usually formed.

... evening before the anthelmintic, the cases were restricted to ... and given 6 drachms of magnesium sulphate on going to bed. ...lmintics used were: three 30-grain doses of β-naphthol in ... the same doses of β-naphthol, with 2 grains of santonin in ...alyptus mixture in 63 cases; eucalyptus mixture and 2 grains ... in 135 cases. The anthelmintic was followed by another

...mmended by BURTON NICOL. *Journal of Tropical Medicine and Hygiene.*

dose of magnesium sulphate, except in some of the cases taking eucalyptus mixture when the motions were free.

The motions were collected for two to four hours after the last purgative; if they had been collected for 24 hours the number of parasites obtained would probably have been larger. From the motions so obtained the worms were recovered by the sedimentation method recommended by Major CLAYTON LANE.* Straining through a fine sieve was found to be unsatisfactory.

The following table gives the main results of the investigation in percentages :—

	Primary examination positive.	Parasites found after anthelmintic.	Parasites found after anthelmintic when primary examination negative.	Total infected.
Fannia sp. (?)	0·0	1·3	1·3	1·3
Tænia saginata, GOEZE	30·0	56·1	28·3	58·3
Opisthorchis felineus, RIV. (?)	17·0	0·0	0·0	17·0
Strongyloides stercoralis, BAV.	18·3	0·0	0·0	18·3
Ascaris lumbricoides, LINN.	46·5	25·7	11·7	58·2
Oxyuris vermicularis, LINN.	1·3	50·0	48·7	50·0
Agchylostoma sp.	(?)	14·8	(?)	14·8
Necator americanus, STILES	56·5	88·7	37·8	94·3
Trichocephalus trichiurus, RUD.	27·0	1·7	1·3	28·3
Entamœba histolytica, SCHAUD., cysts	0·9	0·0	0·0	0·9
Entamœba coli, SCHAUD., cysts ...	6·0	0·0	0·0	6·0
Entamœba sp., trophic stage .	23·1	0·0	0·0	23·1
Lamblia intestinalis, BLANCH	1·0	0·0	0·0	1·0
Trichomonas hominis, DAVAINE	2·6	0·0	0·0	2·6

In the above table the first column indicates the percentage found infected at the primary examination whatever stage of the parasite was found, not necessarily microscopic.

* *Indian Medical Gazette*. June, 1913.

The results given for the protozoa do not represent the full series of 230 cases, as the earlier cases were not examined sufficiently carefully for these organisms, and had to be rejected. The figures for *Lamblia* and *Trichomonas* represent an examination of 195 cases, for *Entamœba* of 117 cases.

Most of the prisoners harboured more than one species of parasite, as the table implies. If protozoa are excluded, 31 per cent. of the cases had three species of parasite, 30 per cent. had four, 14 per cent. had two, 13 per cent. had five, 7 per cent. had one, and 4 per cent. had six; there was a single case with seven species, and a single case, an Indian, where no parasites were found.

The combinations of the parasites were such as might be expected from the relative frequency of the infections. The prisoner with seven species was a Lao who had never been outside the Chiengmai district; he harboured *Necator americanus*, *Agchylostoma ceylanicum*, *Ascaris lumbricoides*, *Trichocephalus trichiurus*, *Opisthorchis felineus*, *Tænia saginata*, and *Oxyuris vermicularis*. Five of these species were recovered after the anthelmintic, including 39 *Necator* and 11 *Tænia*; *Trichocephalus* and *Opisthorchis* were not recovered.

The following are further details of the various parasites found:—

Fannia sp. (?).—A maggot with feathery processes on the segments was found in three cases after the anthelmintic; they probably belonged to this genus, and were too big to have been an infection after the motions were passed.

Tænia saginata.—This cestode was very common; if the cases where only segments were found are put down as having only one worm each, all the infected cases had an average of 2·5 worms each. Heads were recovered in 74 cases, the average number of heads from each being 3·5. From [illegible] man no less than 14 heads were obtained, from another 13, [illegible] three others 11. On one occasion 16 heads were removed post-[illegible] from a prisoner. In about 17 per cent. of the infected cases [illegible] were found at the primary examination. In five instances no [illegible] were found after the anthelmintic, though they had been seen [illegible] liminary examination; these men stated that large masses of [illegible] came away with the purge on the evening before the anthel-[illegible] is possible that in some of them the heads came away first—[illegible] d in a case of mine outside the jail.

While it is not surprising that infection with *Tænia saginata* is common, as various forms of uncooked beef are much eaten by the natives, it is remarkable that *Tænia solium* was not found, as raw pork is eaten even more than raw beef. A similar scarcity of *Tænia solium* is reported from the Philippines.

In this series no other species of cestode was recognised at all, but in an official recently arrived from Bangkok I found cestode oncospheres resembling figures of those of *Hymenolepis nana*. Unfortunately, the adult parasites were not obtained, as no proper examination was made of the motion after giving eucalyptus mixture.

Opisthorchis felineus (?) –In thirty-nine cases of the series, 17 per cent., a very small fluke egg was found at the preliminary examination in scanty numbers. This egg measured about ·027/·016 mm. and had a small boss at the pole opposite the operculum, agreeing in these and other particulars with the egg of *Opisthorchis sinensis*, which it was at first thought to be; but this identification could not then be put to the test, as no flukes were recovered after the anthelmintic in this series. Later, through the kindness of Dr. C. W. MASON, I was present at the post-mortem of a man in whom these ova had been found in the motions in large numbers during the life; some thousands of small flukes were obtained from the liver and gall-bladder. These parasites agreed fairly well with figures and descriptions of *Opisthorchis felineus*. After this I myself had a post-mortem on a prisoner in whose motions these ova had been found a month previously, no anthelmintic having been given on account of the patient's state of health; there were fourteen flukes of the same species in the liver and gall-bladder. In about half the infected cases there was some tenderness to pressure over the liver, chiefly in the gall-bladder region, in no cases marked; some of the cases complained of obscure abdominal pains, while others shewed no symptoms.

Though no fluke was obtained in this series, a second species was obtained later from a prisoner after the administration of eucalyptus mixture; this fluke I was unable to identify, and have forwarded to Dr. LEIPER for determination.

Ascaris lumbricoides.—It will be seen from the table that this worm was recovered after an anthelmintic in less than half the infected cases; in the first ninety cases, where no santonin was given, worms were only recovered from ten out of fifty-six infections. The average number

of worms collected from each infected case was 1·5, the largest number obtained from one case being eight. In children the infection rate is probably higher, and the number of worms per individual much larger, than among adults. I have seen immense tangled masses of these worms recovered from children; one of my patients, a girl 3½ years old, passed 200 of these worms in three days. Sometimes in children *Ascaris* gives rise to symptoms suggesting serious abdominal lesions; in one case I diagnosed acute appendicitis, in another intussusception; the latter was confirmed by a surgeon, who recommended operation; in both these cases the symptoms cleared up at once after passing numbers of worms.

Oxyuris vermicularis.—The ova of this parasite were only found once, at the preliminary examination, and adult parasites twice. The numbers obtained after the anthelmintic were usually small, about six or seven, but on two or three occasions the motions were seething with them.

Agchylostoma sp.—Worms belonging to this genus were found in thirty-four cases of the series, 11·8 per cent., usually in small numbers. From one case eleven worms were recovered, and from another nine; from no other case were more than two worms recovered, the average for all infected cases being 1·8 worms. No attempt was made to distinguish the ova of *Agchylostoma* from those of *Necator* at the preliminary examination, all hookworm ova being put down as *Necator*; this has made no difference to the final result, as in only three cases was *Agchylostoma* without *Necator* recovered after an anthelmintic, and in none of these cases had hookworm ova been found at the preliminary examination.

These worms were at first thought to be *Agchylostoma duodenale;* later it was noticed that they were not typical of that species, and all [illegible] kept—38 out of the original 61, some having been sent to [illegible] and others lost—were gone through again. Of the 38 [illegible] were found to be *A. ceylanicum*, three *A. duodenale*, and [illegible] too badly preserved to form an opinion on; the points relied [illegible] being the distinction between the two species being the very [illegible] teeth and the form of the dorsal bursal ray in [illegible] *um.*

[illegible] *americanus.*—If the stools had been collected for twenty-four [illegible] the anthelmintic, instead of only the first three or four stools,

the infection rate for *Necator* in this series would have been found higher than 94 per cent.; probably every case was infected.

In thirteen cases ova were found at the preliminary examination, when no hookworms were recovered after the anthelmintic; it is improbable that these were ova of *Strongyloides stercoralis*, which are rarely found in the stools; in addition, in only two of these cases was the embryo of *Strongyloides* found.

Other investigations made in Northern Siam also shew a high rate of hookworm infection. Dr. W. H. BEACH, of Nan, found an infection rate of 85 per cent in over 200 cases, mostly children. Dr. W. J. W. MCKEAN, of Chiengmai, examined 242 cases and found 54 per cent. infected; 216 of these were children with an average age of fourteen. Dr. BEACH kindly sent me two worms which were both *Necator americanus*, and Dr. MCKEAN was good enough to give me the opportunity of going over 274 hookworms from his cases; 273 of these were *Necator americanus*, and one, rather damaged, was an *Agchylostoma*, apparently *A. ceylanicum*. There can be no doubt that *Necator americanus* is the common hookworm throughout Northern Siam, and is indigenous to the country, as it is in neighbouring territories.

The average number of *Necators* obtained from each infected case was 22, the largest number obtained from one case being 169.

The hæmoglobin index of 200 cases was estimated with a tinted hæmoglobinometer. While the results given by such an instrument cannot be considered altogether reliable, it may be worth while giving them :—

13	cases	with no	hookworms	had an	average of	90% hæmoglobin.
84	,,	1-10	,,	,,	,,	93% ,,
47	,,	10-25	,,	,,	,,	90% ,,
38	,,	25-50	,,	,,	,,	88% ,,
11	,,	50-100	,,	,,	,,	90% ,,
5	,,	100-150	,,	,,	,,	84% ,,
2	,,	150-200	,,	,,	,,	87% ,,

It is unlikely that any considerable degree of anæmia would result from the small numbers of hookworm found; chronic malaria was probably the chief factor in causing what anæmia there was in this series.

Lamblia intestinalis.—This species was found in two cases of the

series; in one the motions were formed, in the other diarrhœic. I have seen this organism in two civilian patients associated with dysenteric stools; no entamœbæ were found, and in both the *Lamblia* disappeared and the stools cleared up on treatment with β-naphthol.

Trichomonas hominis.—This flagellate was found in five cases. It is very common in loose stools.

Entamœba sp.—Entamœbæ were carefully searched for in 117 cases; among these cysts of *E. coli* were found in seven cases, while those of *E. histolytica* were found in only one; they were plentiful in this case, but mixed with *E. coli* cysts. It is possible that the small number of cases infected with cysts was due to the fact that a laxative was given before the examination, though the motion so obtained was usually formed, or half-formed. Nothing was counted a cyst unless the spores could be distinctly counted. As WALKER has pointed out,* there is another organism, *Blastocytis hominis (?)*, which is apt to be mistaken for an entamœbic cyst; this organism was present in 47 per cent. of the cases examined.

Entamœbæ in the trophic stage were found in 27 cases; they all had the heavy chromatin ring and dense protoplasm, said to be typical of *E. coli*, but they varied greatly in size; some of them may have been the pre-encysted stage of *E. histolytica*.

In cases where there was doubt about entamœbæ or cysts, stained films were made; the method of staining followed was that recommended by WALKER†, a wet hæmatoxylin method, which gave excellent results with the minimum of trouble.

The anthelmintics used.—As already stated, the anthelmintics used were eucalyptus mixture and β-naphthol, either alone or combined with santonin. Without the santonin neither were of much effect in expelling round worms. The average number of hookworms obtained after eucalyptus mixture was somewhat larger than after β-naphthol; with the [illegible] were passed alive, while with the latter they were dead. [illegible]strides of *Tænia* after β-naphthol came away separately, or in [illegible] chains, while the heads were rarely found; after eucalyptus [illegible]proglottides were in long chains, and heads were frequently

[illegible]*ppine Journal of Science*, Vol. vii., Sec. B, No. 4.

[illegible], *loc. cit.*

In this series one death occurred after the anthelmintic, a case of blackwater fever with hæmoglobinuria, bilious vomiting, and jaundice; fever came on within 24 hours of the administration of the anthelmintic, and was fatal in 48 hours. Cases of blackwater fever were occurring at that time among civilians outside the jail, and this death was not at first connected with the anthelmintic. After the completion of this series the treatment of prisoners for worms was still carried on with new arrivals, and among these were two more fatal cases of blackwater fever. These two cases occurred together; in both the symptoms came on within 24 hours of taking the anthelmintic, β-naphthol in both, and death supervened 24 hours later in one, 36 hours in the other. All three of these cases had had frequent attacks of fever, as most natives here have had, and two of them had very large spleens. I have no doubt in my own mind that the anthelmintic was the exciting cause of the attack in all three cases.

I have another untoward event following the administration of an anthelmintic to record. I was asked to see an official's wife who had been unwell for some time with low fever and some diarrhœa. When I saw her she was extremely anæmic, and her temperature was 99° F. An examination of the stools revealed hookworm ova in abundance—in larger numbers than I had ever seen before. Thinking the hookworm might be the cause of her condition, I gave her eucalyptus mixture, but, owing to her weak state, only in half doses. The result of this treatment on the hookworms is not known, as the motions were thrown away. However, within twelve hours her temperature began to go up, and she went safely through a typical relapse of typhoid, without doubt brought on by the anthelmintic.

NOTE.—Since the above was written, Lieutenant-Colonel LEIPER, R.A.M.C., has published a paper, "Notes of the Occurrence of Parasites presumably rare in Man," in the *Journal of the Royal Army Medical Corps* for June, 1915, wherein he refers to some of the parasites sent from Chiengmai, confirming my determination of *Agchylostoma ceylanicum*, Loos, and identifying my undetermined fluke as *Euparyphium malayanum*, LEIPER, and my *Opisthorchis felineus* (?) as *Opisthorchis viverrini*, POIRIER. W. F. J. K.

Dr. C. MORLEY WENYON: I wish to shew two specimens illustrating Coccidia which I have found in cases I have examined recently. During the last two months I have been examining, for protozoal infection, fæces of men invalided from Gallipoli for dysentery or other infection of the gut. I should like to take this opportunity of thanking Dr. G C. Low for his kindness in assisting me in the matter, and for undertaking the examinations when I have been out of town. Dr. Low's assistance has greatly increased the accuracy and value of these observations.

I may say at once that in connection with this work I have examined about 1000 specimens from over 500 different individuals, and out of that total I found that half of them have some form of protozoal infection. The most important point is that out of these 500 individuals, 10 per cent. are carriers of *Entamœba histolytica*. Perhaps I may be excused if I explain the method I adopt, because at the present time this is a very important matter. In making the diagnosis, I rely chiefly on the presence of cysts. The stool on these occasions may be normal, or nearly normal, and may contain no blood or mucus, so that we can only recognise the infection by finding some typical form, generally the encysted form, of *E. histolytica* in the stools. These encysted forms of the entamœbæ really correspond to the eggs of worms, for the finding of encysted forms of *E. histolytica* in the fæces is just as indicative of infection of the gut with this entamœba, as the finding of an ankylostome egg is of an infection of the gut with ankylostoma.

The cysts of *E. histolytica* are quite definite structures, and can be distinguished from the cysts of *E. coli* if one has had some practice in their examination. The important point is that the cysts always breed true, so to speak. If a man commences passing *E. coli* cysts he will do [illegible] many years, and they never transform into cysts of *E. histolytica*; [illegible]larly, if a man passes *E. histolytica* cysts, these again will never [illegible]n into cysts of *E. coli*.

[illegible]men who are carriers of *E. histolytica* are very dangerous, not [illegible]themselves but also to other people. As an illustration of this [illegible] may mention a case I examined some years ago for Professor [illegible]. The man had a tremendous infection of *E. histolytica*. The [illegible] was identified by finding large numbers of the characteristic [illegible]ms fæces. In spite of advice he started abroad, and at Port Said

he had a relapse in the form of an acute attack of dysentery, followed by liver abscess, from which he died. Such a case would have been saved if treated with emetine. Another case of the kind was a lady missionary returned from India. I found her to be passing large numbers of *E. histolytica* cysts. She had no dysentery at the time, but by means of her histolytica cysts I was able to produce an infection in cats, and I was able to pass the infection on from one cat to another. All these cats had typical amœbic ulcers of the gut. Eventually one cat developed amœbic abscess of the liver, and thus, starting from a carrier case, one had produced in cats the whole picture of the amœbic infection in man, with its complication of liver abscess.

The other day, I saw an interesting case of a man who had contracted amœbic dysentery, though he had never left England. He was working in the Docks on a transport. He was admitted into the London Hospital with an attack of dysentery. This cleared up. Soon after he was found to have liver abscess. This was opened and drained, and I found *E. histolytica* in the pus. I examined the stool, and found nothing on the first occasion. I again failed on the second examination, but on the third occasion I found typical cysts of *E. histolytica* in large numbers. That man was infected with *E. histolytica* in this country, very likely from a man coming home from abroad and carrying the infection. Unfortunately he died.

Of all the Gallipoli cases I have examined, 10 per cent of the 500, *i.e.*, 50 of them, are carrying *E. histolytica* infection. These men are dangerous to themselves, for they may relapse and get liver abscess. They may go back to a country like Gallipoli, where the conditions of spread of this disease are favourable, and they may be the means of spreading amœbic dysentery out there. These men ought, if possible, to be identified as carriers, and should be treated with emetine.

Other protozoal infections are not so important. Fifty per cent. are carrying *E. coli*, and a smaller number one or other of the flagellates. It is interesting to note that two parasites, which we know little of as regards their presence in man, have turned up in these Gallipoli cases. Dr. Woodcock identified a structure in a man at St. George's Hospital as a coccidium, and I was able to demonstrate that it belonged to the genus Isospora. Last night I was examining specimens, and I came across a structure which proved to be a totally different coccidium, belonging to

the genus Eimeria. One common coccidium of this genus is parasitic in the liver of rabbits, but the one I am shewing to-night is like the coccidium of the mouse intestine. The Isospora has been found in about ten out of the 500 cases I have examined. The other I only found last night in one case. Infection with coccidium is spread by individuals eating the oöcysts after they have completed their development on the ground. Of the type of infection they produce we really know nothing at present.

Dr G. C. Low: I think one point of special interest occurs in Dr. KERR'S paper, and that is the toxic effects of the β-naphthol in some of the cases.

The drug might, of course, have acted as the exciting agent of a real attack of blackwater fever, but it seems much more likely that the hæmoglobinuria had nothing to do with blackwater fever, the condition rather being due directly to the naphthol. I recently gave a man three 20-grain doses of β-naphthol, and this produced toxic symptoms, namely, lethargy, a semi-comatose condition, and also spitting of blood. These conditions, alarming while they lasted, passed off on the second day, and the patient made a perfect recovery. There was never albumin in the urine, and the kidneys seemed to escape entirely. If one uses these large doses, namely, three 30-grain doses, as recommended by BURTON NICOL, then in susceptible individuals we must expect sometimes to get toxic symptoms.

As regards the case of amœbic dysentery and liver abscess, mentioned by Dr. WENYON, in a man who had never been out of England, such cases have been reported before. Major MARSHALL reported one in a man near Edinburgh, who was supposed to have got his infection from a soldier who had returned from India, and SAUNDBY and MILLER have also reported one from Birmingham. Such cases are of great interest, and shew that if amœbic carriers are introduced into England there is a possibility of them spreading dysentery amongst the indigenous population.

Miss MURIEL ROBERTSON: I should like to ask what evidence there is of treating amœbic carriers with emetine, and whether it is successful in getting rid of the *E. histolytica* cysts or not? The literature on the subject draws attention to the fact that cysts are relatively resistant

to emetine, and I would like to know if there is any clinical experience as to how far carriers, as apart from men suffering from acute attacks, can be treated with emetine.

Dr. G. C. Low: The point raised by Miss Robertson is one that requires further elucidation. Certainly, in some cases, proper doses of emetine cause the disappearance of *E. histolytica* cysts from the stools. I do not see why they should not disappear, because if the drug kills the vegetative form from which they arise, then no more cysts can be produced. The condition is similar to that of the crescents in malaria. Quinine has no effect upon these *per se*, but by killing the young forms which produce the crescents the latter then ultimately disappear. It is possible that the cases described, where the cysts have resisted the emetine, have not had proper doses, or in some way or other the amœbæ producing them have not been killed by the emetine. However, the subject requires more work and careful study. I am following up this line of research in several cases at the present moment, and hope soon to publish some definite results.

Dr. Andrew Balfour: It may be of interest, if I state that Captain Archibald, working at Lemnos, found in about 5 per cent. of the cases he examined that *Trichomonas intestinalis* was present. The pathogenicity of these flagellates is not definitely determined, and although found in 5 per cent. of cases of dysenteric diarrhœa, that is not to say that this number were suffering from trichonomiasis. I have, however, seen some cases in which there was a very definite form of diarrhœa. It is characterised by the patient passing bright yellow stools in large quantities, and may or may not be associated with colic. Such cases would certainly appear to be due to trichomonas infection.

I think Dr. Wenyon's results are very interesting indeed. When I came back from the East I was surprised to find that histolytica cysts had hardly been found at all, but I thought this was probably due to emetine being so universally employed in the Gallipoli Peninsula. Even so, it seemed to me curious that so few histolytica cysts were being discovered. I now think this was largely due to the fact that the men who were examining the stools in these cases were not sufficiently familiar with the parasite of amœbic dysentery. We all know that

Dr. WENYON is an expert in this line of work, and it is, therefore, interesting to note that when he started his examination he began to find that histolytica infection was present.

I think, as regards histolytica infection, the number of cases of true amœbic dysentery in Gallipoli has been very considerable. It must be 60 or 70 per cent. of all cases, although, doubtless, in many instances, there has been mixed bacillary and amœbic infection.

Turning again to trichomonas infection, the Commission, of which I am a member, had its attention directed to the question of water supply at Mudros. The water was brought from Egypt by ships, and taken to the shore in tank barges. Remembering the work of ESCOMEL in Peru, who apparently traced an outbreak of flagellate diarrhœa to trichomonas in certain water tanks that had not been cleaned out for a long time, the Commission had a look at the water-tanks at Mudros East, and found that in some of them a good deal of slime had collected, as it had not been possible to clean them out for some time. Samples of the slime were taken, and they were submitted to examination by Captain ARCHIBALD, who found a flagellate present, which he regarded as a trichomonas. The tanks were cleaned out, but with what results I am unfortunately unable to say.

Dr. R. P. COCKIN: With reference to Dr. LOW'S remarks on the toxic symptoms observed after treatment with β-naphthol, it may be of interest to record that, in a series of 1,400 cases of ankylostomiasis treated in the West Indies, I met with five cases such as those to which Dr. KERR calls attention.

The urine in these cases was identical in appearance with that which one sees in cases of blackwater fever, but differed from it on spectroscopic examination; the spectrum shewn in β-naphthol poisoning being similar to, if not identical with, that which is observed in cases of hæmatoporphyrinuria.

On the appearance of this complication, the use of β-naphthol was at once stopped, and the patient was placed on drachm doses of ext. ergotæ liq. thrice daily. The urine resumed its normal colour in from 24-72 hours, and there was no mortality.

Upon resumption of the treatment for the expulsion of the worms,

I thought it advisable to use thymol, and in the five cases recorded this was done, without any return of the dark-coloured urine.

The PRESIDENT: I understand from Dr. COCKIN'S remarks that the chemical condition of the urine he saw was similar to that occurring after sulphonal poisoning. Fellows of this Society will recognise that you may poison a man, as seen by his renal excretion, but that he has not necessarily the symptoms of true blackwater fever. It is very difficult, in a country where malaria is present, to be sure which condition is present, and Dr. KERR says there were cases outside getting blackwater fever at the same time.

With regard to Miss ROBERTSON'S question, I think that our rule in this country—in fact, in every country, I should say—is, having found evidence of amœbic dysentery, to give emetine. That must be the clinical rule until we know something better. Exceptions are possible where the patient has had large doses of emetine already, which you may or may not continue. I would remind Miss ROBERTSON that every case that travels from one country to another—say, from Gallipoli to Egypt or to England—where cysts are discovered in the fæces, is a carrier. That is probably the opinion of every Fellow in this room, though it is not the opinion of the whole of the profession generally. Many would say if you had an attack a month ago that it is all over. The carriers after an attack of dysentery are the people from whose fæces cysts can be obtained. Emetine will get rid of amœbæ if you go on long enough, and by the intracellular method. It is probably not so good by the mouth, as I do not think you can get at the organisms inside the bowel by giving it by this route. I agree with Dr. Low that we have not had enough cases yet to be perfectly certain how many of these carrying cysts can be cured by prolonged doses of emetine. We do not know how many doses of emetine you are to give, nor the exact dose in grains, for such a purpose.

I am told there is a paper coming out to-night in the *British Medical Journal*, by Dr. DALE, which states that emetine may be a dangerous drug when given in ordinary doses over a prolonged period to cats and rabbits. With humans I have not yet met with signs of poisoning when 1-grain doses were given, nor have I noted any cumulative effects. I have

seen cases published where emetine in 1-grain doses was taken for three months at a time, with no untoward results.

I should like to congratulate Dr. WENYON on the work he has been doing.

We are in a condition of some ignorance about this Dardanelles type of dysentery, and the first thing to do is to find out by means of examining the fæces what organisms are present.

Another point of interest is to know how the flagellates and amœbæ get into the body, as, once being certain of this, we can then take the necessary steps to prevent them, and so diminish the disease.

THE CAUSATION OF SO-CALLED TROPICAL ANÆMIA.

BY

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Every one with tropical experience will agree that Europeans, who have lived long in the tropics, usually become ænemic-*looking*. At one time it was usually tacitly assumed that this appearance denoted true anæmia, *i.e.*, an amount of hæmoglobin in the blood below the normal. Estimates of the amount of hæmoglobin in the blood do not, however, support this view. The amount of hæmoglobin, if reduced at all in the tropics, is not reduced sufficiently to give rise to a definite anæmia.

It is sometimes suggested that the anæmic appearance is due to ischæmia of the skin capillaries,* *i.e.*, to a contraction or diminution in the number of the skin capillaries causing a diminution of the quantity of blood in the skin. I am not aware that this ischæmia has ever been demonstrated, or that there is any independent support for this view. On *a priori* grounds it is very unlikely that there is a relative absence of blood in the tropics. It is undoubted that the chief skin function, sweating, is more marked in the tropics; and it is unlikely that the blood supply to the skin diminishes with this increased functional activity. It is a general physiological rule that the blood supply to an organ increases when the organ is functionating. Moreover, it is generally held, and I think quite correctly, that the heat regulating mechanism requires that in the tropics the cutaneous vessels shall be dilated.†

The cause of a person's skin having a particular colour, is by no means such a simple matter as it may appear at first sight. Before any scientific discussion on skin colour can take place, it is necessary to clearly understand the optical factors which influence the colour.

From the optical point of view, the skin consists of three layers: —

1. The unpigmented outer epidermis, more or less transparent.

* *Manual of Tropical Medicine*, CASTELLANI & CHALMERS, 1913, p. 101.

† *Ibid.*, p. 84.

2. The pigmented layer more or less absent in the case of Europeans living in a temperate climate.

3. The true skin with its blood vessels.

Again, from the optical point of view, ordinary white light really consists of a mixture of light of all colours of the spectrum, and the colour of an object depends on the relative amounts of these different coloured lights which the object sends back to the eye of an observer. A white transparent object, such as glass, allows practically all the light to pass through unchanged. A white opaque object, such as chalk, scatters most of the light in all directions, and appears white because light of each colour is scattered to the same extent. If one colour is scattered to a greater extent than the other colours, the object appears tinged with that colour. It is by the light which is scattered by the various layers of the skin that we judge the colour of the skin. But we must remember that if either of the outer layers of the skin are opaque to light of any particular colour, then light of that colour cannot be scattered by the deeper layers, since light of that colour will never reach the deeper layer.

The problem we have to consider then, is the effect which various changes in the three optical layers of the skin will produce on the light scattered by the skin back to the eye of the observer, and more especially why there is less red light scattered by the skin of Europeans in the tropics than by the skin of a European living in Europe.

It will be convenient to consider the deepest layer of the skin first, *i.e.*, the layer with blood capillaries. The red colour of the skin depends mainly on the amount of red light which reaches this layer, and is scattered by it back to the eye of an observer. A decrease in this will be brought about either by a decrease in the quantity of blood in the capillaries or by a decrease in the quantity of red light, which gets through the epidermis to this layer. I argue that there is no decrease in the quantity of blood in the capillaries, hence the white colour of tropical skins must depend on the smaller quantity of red light which reaches the capillaries, and is scattered there back to an observer's eye.

A decrease in the quantity of red light, which reaches the capillary layer, can be caused in three ways :—

1. There may be a greater thickness of the unpigmented epidermis,

which (unless the epidermis is perfectly transparent) will allow less light of all colours to pass through to the capillary layer, and will, at the same time, scatter more white light back to an observer's eye.

2. Without change in the thickness of the unpigmented layer of the epidermis, it may become more milky, so that, as before, less light reaches the capillary layer, while more white light is scattered back to an observer's eye.

3. The epidermis may become partially opaque to red light by the deposition in it of a pigment opaque to red light.

We may sum up by saying that the anæmic appearance of the skin of a European in the tropics must be due to the epidermis becoming either thickened, more milky in colour, or more opaque to red light. I have no doubt but that variations in the thickness and milkiness of the epidermis do occur under different circumstances, and that these factors do influence skin colour; but the third factor, opaqueness to red light, is, I believe, the predominating factor.

Let us assume that the epidermis becomes partially opaque to red light. Then the light which is scattered by all except the most superficial layer of the epidermis will be deficient in red rays. But if this deficiency is just made up for by the red rays which are scattered by the blood in the capillaries, the skin will appear white in colour. This is, I believe, the true cause of so-called tropical anæmia.

In point of fact, I do not suppose that it is the red light only which is absorbed by the epidermis; the blue, violet, and green are also absorbed to a greater extent than the yellow; but it is the relative opacity of the epidermis to the red rays of light which annuls the red colour of the blood capillaries, and causes the apparent anæmia. Of course, the only way the epidermis can become partially opaque to light of any particular colour is by the deposition of pigment in the epidermis. In other words, an anæmic look coincides with the very first stage in the production of pigment in the epidermis. Such production of brownish pigment is, of course, admitted by everyone in the case of dark races, and also in the case of Europeans who are much exposed to the sun: in the case of Europeans the resulting colour is usually complicated by "erythema solare."

There is one factor which for simplicity I have left out of the above discussion. The appearance of the skin may vary according as to

whether the pigment tends to be concentrated in the more superficial or in the deeper layers of the epidermis, unless, indeed, the epidermis can be regarded as perfectly transparent. If the pigment is concentrated in the superficial layers, little light will penetrate the epidermis, and little light will be scattered there, and the skin will appear black. If the pigment is concentrated deep in the epidermis, and if the superficial layers are able to scatter light, the skin will then appear of a lighter colour.

There are certain well recognised instances when white light, or a white colour, is made from coloured light by absorbing the colour which is in excess. For satisfactorily viewing microscopic slides stained with hæmatoxylin and eosin, white light is required. An oil lamp gives out yellow light in excess, and, in consequence, we insert a blue glass, which is relatively opaque to the yellow rays of light, and, as a result, we get a whiter light. Again, linen, after use, tends to become tinged yellow, and a good laundryman knows that he can correct this by lightly staining the articles with "blue," which is relatively opaque to yellow light In the case of a European's skin it is the red rays which are in excess. In the tropics these are absorbed by a small amount of pigment in the skin, and so the skin appears whiter. When the pigment appears more plentiful the skin, more or less, assumes the colour of the yellow-brown pigment or pigments.

The change which takes place in the skin can also be imitated in the following way. If a deep-red coloured piece of paper, such as the 100 per cent. red on a Tallquist hæmoglobin scale, is placed on a table and looked at in a strong light through a piece of slightly transparent paper, the red tint will be visible, just the same as the red tint of a European's skin is visible. But if we insert between the red paper and the slightly transparent white paper a piece of black paper, or a piece of paper opaque to red rays, the red tint will be removed, and we shall only see the white or grayish colour of the slightly transparent paper.

A METHOD FOR THE TRAPPING OF *GLOSSINA MORSITANS* SUGGESTED FOR TRIAL.

By J. O. SHIRCORE,

Medical Officer, East Africa Medical Service.

It is not proposed to discuss at length why the method described below may be useful; a short outline is sufficient to indicate that such a method may be worthy of trial. It is based on the known habits of this tsetse, described and reiterated time and again for many years.

(a) Attraction for colour: dark colours, such as dark blue, dark gray, etc.

(b) The attraction of the fly by moving objects. The light structure and the shape of the traps are particularly designed to catch any prevailing wind, in order that the traps may be thrown into movement.

(c) The "following range" (Dr. HODGES).

(d) The habit of *G. morsitans* of indulging in short periods of intermittent flight and rest.

(e) The dispersal of the fly when the object they are following comes to rest.

(f) The fact that female *G. morsitans* are more frequently taken on moving objects than otherwise (Sergeant-Major GIBBONS).

(g) The large area of adhesive surface exposed: the diagram shews forty traps, which represent 240 square yards.

(h) The marked preference for forest in contrast to their dislike for open plains and cleared spaces.

(i) The concentration of *G. morsitans*, for various reasons, at certain times into definite areas during the height of the dry season, August and September, in the Nyasaland Proclaimed Area.

(j) That no special decoys are necessary, insomuch that the natives of the district would act as such while travelling. The traps could be placed at suitable positions, so as to act as "interceptors." The space between the traps—*i.e.*, one yard in every direction—is sufficient to allow of free passage through these systems. Men or animals may be used as decoys to entice the fly into these situations, and a slow pace should

be observed while going through, if not an actual halt in the middle for a short while.

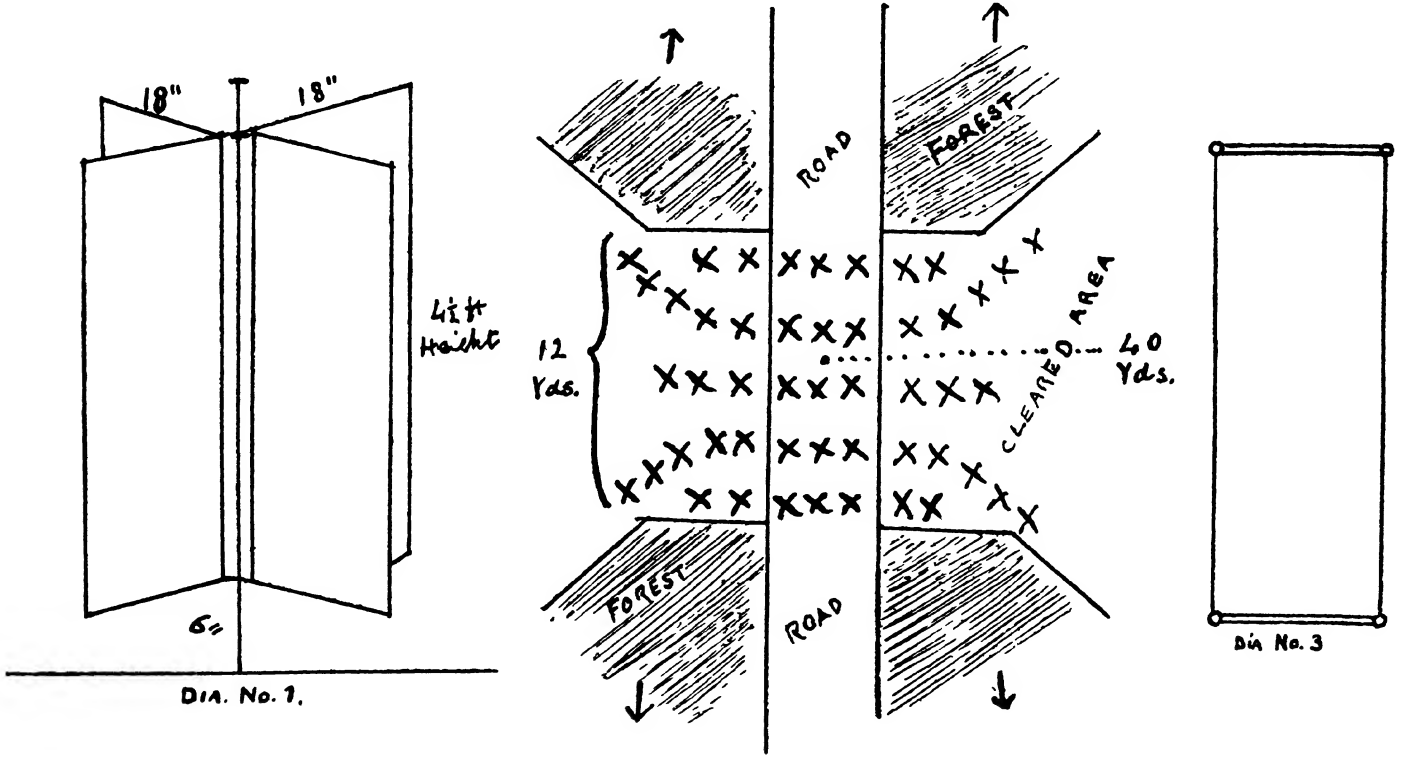
(k) That this method may possibly be found of value when employed for the purpose of controlling the extension of fly-belts along traffic routes into fly-free country.

The trap I suggest may be best made of light canvas, in lengths of 4½ft. by 1½ft. wide, and of any suitable dark colour. The ends of the canvas sewn—*vide* diagram No. 3—so as to slip over light rods of iron. Four pieces of canvas to go to one trap, and the surface adhesive on both sides; the capturing area of each trap being six square yards. The light rods should be placed at right angles to each other, top and bottom, and adjusted so as to pivot round on the central support, which must be stout enough to prevent the trap from being blown over. About six inches should be allowed between the lowest part of the canvas and the ground level.

The arrangement of the traps can be modified as desired, and the most useful formation discovered by experiment, and added to or reduced in number; but the distribution demonstrated in diagram No. 2 may prove to be a good model to begin work upon.

Before the traps are erected an area should be thoroughly cleared, as indicated in diagram No. 2. I would suggest forty yards on each side, measured from the centre of the road or path, and not much more than twelve yards for the length of clearing for the trap-covered area; if too much is cleared all the fly may not follow through. The shaded area in diagram No. 2 is forest. Numerous suitable sites exist in the Proclaimed Area, Nyasaland, and one in particular situated just before the path from Kambwiris village leads on to Kuti Dambo.

Very little clearing would be required in some places, and the whole experiment would not cost more than a few pounds.



Diagrams of Traps described by Dr. J. O. SHIRCORE.

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ETIOLOGY OF TYPHUS.

BY W. J. PENFOLD, M.B., D.P.H.
From the Lister Institute.

PRELIMINARY COMMUNICATION.

Two main purposes cause me to desire to deal with the subject of this evening's paper.

I would, first of all, like to utter a word of caution as to the acceptance of the claims made by PLOTZ for a bacillus which he believes to be the cause of typhus. His preliminary communication (1914) contained a series of statements relative to this bacillus which caused me to abandon, for a time, the personal work I desire to present to you now.

1. In that communication he stated that his organism occurred in the blood of 11 out of 11 cases of typhus examined; six of these cases being epidemic typhus, and five being endemic cases—the so-called BRILL'S disease. In a second paper (1915), he says that in a total

of 34 cases of BRILL'S disease examined in the febrile stage, he has obtained his organism in 18 cases. If from this series we deduct the five cases of the first series, we find that he had 13 successful blood cultures in 29 cases: that is, his first series shewed 100 per cent. successes, and his second, 45 per cent., approximately.

2. In his first paper, the organism was alleged to be anaerobic on isolation, and to become aerobic on cultivation; in his second paper this is denied.

3. PLOTZ, in his first paper, described bactericidal substances in the blood of typhus patients, while in his second paper it is stated that bacteriolysins and bacteriocidins could not be demonstrated in immune typhus sera.

4. In his first paper he stated guinea-pigs reacted to injection of his organisms by a fever which set in 24 to 48 hours after injection, and lasted four to five days, and then suddenly ceased. In his new paper, two guinea-pigs are described as reacting specifically to the injection of his organism, but in neither case did the type of fever bear any resemblance to that described in his first paper.

5. In the first paper he stated that the antigen made from the bacillus from cases of BRILL'S disease binds complement in the same manner as the antigen made from the bacillus isolated from cases of typhus fever. In the second paper this is shewn to be only partly correct.

The first paper of PLOTZ was short, and contained but few facts, many of which are gravely challenged in his second paper. It may appear an invidious task to criticise the work of a fellow worker in this way, but this particular work has been accepted so generally, especially by American bacteriologists, that it has doubtless held up many earnest workers in their research, and I feel it incumbent upon me to state frankly that I see grave difficulties in the way of accepting, at present, any of the results recorded by PLOTZ in either of his papers. Without [illegible]ing further the discrepancies between these two papers, I might [illegible] to examine the results of the second paper. The organism, [illegible]robic rod, appears to have been found in the blood of a large [illegible] of typhus cases. As a cultivation medium for the organism, [illegible]ucose agar, with or without kidney tissue, gave the best results. The strains isolated were tested in respect of fermentation powers on

ten media, and all gave identical results. The blood of 198 cases not suffering from typhus were examined by the same technique, and none of them shewed the same organism.

The specific organism was isolated from an artificially infected monkey, and also from eight guinea-pigs so infected. These facts would be impressive if they were confirmed from independent sources.

OLITSKY, a co-worker of PLOTZ, who did the serum reactions with the PLOTZ bacillus, found support for the view that it was the causal organism of the disease. He obtained positive complement fixation tests with patients' serum, using epidemic, endemic, and mixed antigens. He likewise obtained positive agglutination results with the serum of typhus cases against the PLOTZ organism, but against no other. Since, however, the other strains tested appear to have been confined to those resembling the PLOTZ bacillus, this observation does not have great value. Various workers have shewn that non-specific agglutinins, *e.g.*, those for *B. typhosus*, may be demonstrated in relatively high dilutions of the serum of typhus cases.

In the section of the paper dealing with the animal experiments, two guinea-pigs inoculated with the culture are described as shewing fevers which were alleged to resemble typhus fever reactions. Since one of the animals was not protected against virulent typhus blood, and the other was killed during the course of an atypical fever, no satisfactory deductions can be made from these experiments in favour of this organism being the cause of the disease. A large number of similar experiments gave a negative result in respect of fever.

In contradistinction to the work of PLOTZ, I would like to draw attention to the work of WILSON, of Belfast, who described cocci as occurring in the blood of typhus fever. In 15 out of 33 cases examined, he obtained from the blood a diplococcus. The cocci were frequently identical, but sometimes shewed slight variations amongst themselves.

This percentage is as good as that of PLOTZ'S second series in BRILL'S disease, and I feel that this coccus should not be set aside in the meantime as a secondary invader, as suggested by WILSON. Similar cocci had been described before by various careful workers.

HORT and INGRAM (1914) found a cocco-bacillus regularly associated with typhus fever. They found it in the blood, urine, and cerebro-spinal fluid. The cases they investigated were chiefly in Belfast. At the

instance of Dr. HORT I went to Belfast (1914) to investigate a recrudescence of this disease, and, if possible, to isolate the cocco-bacillus. I failed to find this cocco-bacillus in material from any of the cases. The delay in reporting my positive results was due to the fact that PLOTZ announced the discovery of the specific cause of the disease. A comparison, however, of his two papers has caused me to think that the cause of typhus fever is still an undecided question.

PERSONAL RESULTS.

Clinical material.—Four cases only were examined. The first two cases were convalescent; the third case was, at the time the specimens were taken, in the crisis; while the fourth case was acutely ill, and had not yet passed through the crisis. (The charts of the cases are annexed.)

Technique.—Blood was taken from each patient from the median basilic vein, after careful sterilization of the skin. It was added to an equal quantity of sterile citrate solution, and taken straight away to the laboratory, a distance of about four miles, where animal inoculations and cultivation were at once carried out. Smears of the blood were made at the bedside and examined by GIEMSA'S method. Samples of urine were drawn off by means of sterile catheters into flasks and plate cultures of the samples made the same day. The deposits obtained by centrifuging the urine were examined by smear and cultivation methods.

Media used for cultivation experiments.—Agar, serum agar, human blood agar, broth, serum water, serum agar shake cultures, and serum agar tissue shake cultures. The results obtained shewed that human blood agar was the most useful medium. It never failed to shew growth when any other medium shewed growth. Serum containing media were of use, but not so useful as the fresh human blood agar; agar, broth and serum water gave negative results. Fresh human blood agar gave the best results on plates. The bloods and urines yielded cultures on five occasions, and they were on each occasion pure cultures of a coccus.

SYNOPSIS.

Cultivation results examined after 48 hours' cultivation.—Case I., convalescent, yielded no cultures. Case II., likewise a convalescent, [illegible] no culture from the blood, but a pure plate of a coccus was grown [illegible] the urine. The bloods of Cases III. and IV., on being plated on

peptone blood agar, gave pure plates of green colonies of a coccus about to be described.

The plates from the urine and blood of Cases III. and IV., which were made on human blood agar containing no peptone, shewed likewise pure plates of the above-mentioned coccus, but the green ring surrounding the colonies did not appear on the blood agar plates in the absence of peptone.

All the five cultures isolated gave, on human blood peptone agar after 24 to 48 hours' incubation, a colony of fair size, with a marked green halo around it. The colonies were numerous on some of the plates. Morphologically the cocci were arranged in pairs or tetrads. They were gram positive.

Subcultures from the blood agar on to ordinary agar gave a very delicate discrete or confluent growth, while subcultures on serum agar gave a denser confluent growth. The morphology on these slopes was essentially tetrad, and some of the tetrads on the serum agar shewed capsules. All the cultures were gram positive. The organism grew at room temperature without liquefaction of gelatine. The coccus is not injured by a prolonged exposure to low temperature, viz., + 2° C. It failed to grow on wort agar at 37° C. in 48 hours. The fermentation properties of the five strains were tested on 21 carbohydrate media. They all gave identical fermentations reactions, viz. :—acid without gas on glucose, laevulose, mannose, galactose, maltose, lactose, saccharose, raffinose, dextrin, inulin, amygdalin, salicin, glycerine and mannite. The acid reactions in the case of the positive alcohols, polysaccharides and amygdalin, were relatively slow in developing. Arabinose, isodulcite, erythrite, adonite, sorbite, and dulcite, gave no acidity within seven days' observation. All the strains gave acidity in milk. Some time after isolation, these strains were not pathogenic for mice, rabbits or guinea-pigs. These results are a little different from those obtained by WILSON. His strains did not ferment inulin, amygdalin, or salicin, and were irregular in their action in mannite, some being positive, and some negative; moreover, his strains clotted milk. He does not give the nature of the growth on blood agar, so that one cannot compare the results on this point.

Agglutination reactions of the strains were attempted.—A rabbit was inoculated four times with one of the coccal strains derived from the urine of Case No. IV. This strain was not agglutinated by a 1/10 dilution of

the serum of the rabbit in question, before immunization; after the fourth injection, strains from all three cases were tested against the serum, when they were all found to be agglutinated fully after one hour in 1/50 dilution, and slightly in 1/100, after the same interval.

Inoculation of monkeys with patient's blood.—All inoculations in this series were intraperitoneal. The patient's blood was added to an equal quantity of citrate solution. *M. rhesus* and *M. cynomolgus* were the species used.

Monkey	4A	given	3 c.c.	of citrated blood mixture of Case IV.
,,	4B	,,	,,	,,
,,	4C	,,	,,	,,
,,	3A	5	,,	Case III.
,,	3B	,,	,,	,,
,,	2A	6	,,	Case II.
,,	1A	,,	,,	Case I.

The temperature reactions resulting from these inoculations are indicated in the accompanying charts.

In taking samples of blood from the monkeys, for blood culture or for further propagation of the disease, an anæsthetic was given to the animal, the carotid exposed, and the needle of a syringe inserted into the vessel to withdraw the blood; the vessel was then ligatured. All the wounds appeared to heal by first intention. It is exceedingly difficult to insert a syringe needle into any subcutaneous vein in small monkeys, and in drawing blood by cardiac puncture death occasionally ensues. After anæsthetizing and withdrawing a small sample of blood from the carotid, a sharp fall of temperature frequently occurs, but this fall is evanescent.

The effects produced by the inoculations of the patient's blood into monkeys are summarized in the following table:—

Monkey 4A, Pat. IV.—6 days' incubation; 8 days of fever, with a remission; fall by lysis; recovery.

Monkey 4B, Pat. IV.—6 days' incubation; sudden rise; 8 days of fever; critical fall; relapsing type of fever.

Monkey 4C, Pat. IV.—6 days' incubation; gradual rise; 8 days of fever; irregular fall; no relapse.

Monkey 3A, Pat. III.—13 days' incubation; 3 days of fever; fall by lysis; hypothermia and death.

Monkey 3B, Pat. III.—9 days' incubation; 12 days of fever, gradually rising to height and gradually falling.

Monkey 2A, Pat. II.—No definite fever.

Monkey 1A, Pat. I.—11 days' incubation; 10 days of fever, with a remission; fall by crisis.

Patient No. IV. was acutely ill. All the monkeys inoculated with her blood agreed in having an incubation period of six days, with eight days of fever. The mode of onset of the fever, the acuteness of the crisis, the presence of a remission of the fever or of a relapse, varied in the different monkeys; but all the types of fever shewn agree with those described either by NICOLLE and his co-workers, ANDERSON and GOLDBERGER or RICKETTS and WILDER. ANDERSON and GOLDBERGER, in a large series of monkeys, found that the duration of the fever was usually eight days..

Patient No. III. when the blood was taken from her was at the crisis, and the two monkeys had longer incubation periods. The fever in monkey No. 3A was acute and short lived, and after a marked crisis hypothermia set in and the animal died. This type of typhus fever reaction, followed by hypothermia, has been observed by various authors, in monkeys. Monkey No. 3B had a long continued fever, reaching its height slowly and falling slowly.

Patient No. II. (ninth day of convalescence).—Monkey No. 2A only had the blood of this patient injected, and no definite fever resulted.

Patient No. I. (third day after crisis).—Monkey No. 1A injected from patient No. I. had, after an incubation period of 11 days, 10 days of fever with a remission. The fall of temperature in this monkey was by definite crisis. It has already been shown (GOLDBERGER and ANDERSON) that the virus of experimental typhus of the monkey persists in the blood after the crisis. Apparently the same applies to the human subject.

Natural immunity in the monkey to first injections of human typhus blood is present in 38 per cent. of cases, according to ANDERSON and GOLDBERGER'S table, or if this table be corrected by the exclusion of eight animals which were unsatisfactory from the experimental point of view, 30 per cent. are still found to be immune. In the above short series we had no immune monkeys apparently.

No attention is drawn to the clinical condition of the monkeys apart

from the fever reaction, as it is agreed that this clinical condition is not of any value in the diagnosis of typhus in the monkey. No rash was observed in any monkey, though a diffuse redness of the face was observed in one case.

Blood smears were made from the bloods of all infected monkeys and stained by GIEMSA'S method; no organisms of any sort were discovered in them.

Results of blood culture in the case of those monkeys inoculated with patient's blood.—Monkeys Nos. 4A, 4B, and 4C were all bled at the crisis. The blood was plated out on the surface of human blood agar plates, all of which remained sterile, though observed for several days. Monkey 3A was likewise bled at the crisis, and monkey 3B slightly after the middle of the febrile period. The blood was immediately added to agar at 44° C. and poured into agar plates. In the case of both these monkeys each of the plates shewed a few colonies of cocci, which were morphologically indistinguishable from the coccus isolated from the patient's blood. The coccus from monkey 3B was put through its fermentation tests, and agreed in these tests also with the coccus obtained from the patients.

Monkey 2A never had fever, and was consequently never bled.

Monkey 1A was bled about the middle of the febrile period, but all the plates inoculated with the blood remained sterile.

It appears therefore that the blood of the monkey may appear sterile at the height of the fever; as tested by the growth on aerobic blood agar plates. From No. 3B, however, we cultivated the patient's coccus. In no case did any bacillary colonies appear on the plates.

Inoculation of monkeys with blood of infected monkeys :—

Monkey No. M_1 was given 6 c.c. of citrated blood of monkey No. 4C.

„ No. M_2 „ „ „ „ „ No. 4B

The blood mixture was 1 : 1 blood and citrate solution, and it was injected subcutaneously. The accompanying charts give the temperatures following these inoculations.

Monkey 4B was in the crisis when bled, and the inoculation into monkey M_2 gave a negative result.

Monkey 4C was likewise in the crisis when bled, but the inoculation of its blood into monkey M_1 gave a mild fever, followed by a period of hypothermia, from which the monkey recovered.

Monkeys inoculated with cultures isolated from patients.—Monkey

C_1U_2 was given an intraperitoneal injection of a quarter of a loop of a peptone blood agar slope of the coccus isolated from the urine of patient No. 2. No fever resulted.

Monkey C_2B_4 was given a subcutaneous injection of the coccus obtained from the blood of patient No. 4. The culture used was grown on blood agar containing no peptone. Fever resulted on the sixth day after injection, it lasted for twelve days, when it remitted one day, the remission being followed by an irregular, probably septic, temperature.

This monkey possibly suffered from typhus.

Monkey C_3B_4 was injected intraperitoneally with a portion of a peptone blood agar culture of the coccus obtained from the blood of patient IV. This monkey shewed mild fever after six days of incubation. The fever lasted, with a remission, eleven days, after which the monkey developed a very low temperature and died.

The experiments on the effects of cultures on monkeys were then interrupted by the war, to be resumed a few months later.

The organism was then grown on a blood agar slope, containing about 50 per cent. of fresh human blood. This culture was added to a human blood citrate mixture, and inoculated into monkey C_4B. No fever resulted in eleven days. A similar experiment to the last one was carried out, only the organism was grown on fresh human blood agar without peptone. This culture was given to monkey C_5B. No fever resulted during the following twenty-five days. The organism was likewise grown for eighteen hours on filtered horse serum, and 2 c.c. of this culture were given intraperitoneally (in 5 c.c. of a mixture of equal parts of fresh human blood and citrate solution) to monkey C_6S. This citrated blood mixture was used to imitate as closely as possible the experiments with citrated virulent typhus blood. This monkey developed no fever.

We see, therefore, that two out of three monkeys inoculated with this organism, immediately after isolation, gave fever reactions starting on the sixth day after inoculation, which reaction possibly indicated typhus, while three cultures inoculated into monkeys, some months after the isolation of the organisms, had no effect on the temperature.

From the blood of monkey M_1 a gram positive coccus was isolated similar, in so far as it was examined, to the coccus obtained from the

patients. It was inoculated intraperitoneally into monkey C_7M, but failed to produce fever in twenty-six days.

The results obtained by inoculating cultures into animals raise a very important question, viz.: To what extent does the absence of fever reaction, after inoculation of a culture, militate against the possibility of the particular organism being the cause of typhus. No fact relative to the etiology of typhus is more widely accepted than the fact of louse transmission, but lice, presumably infected, do not, as a rule, produce fever in monkeys, though they do confer immunity upon them.

The most probable explanation of this may not be, as suggested by PLOTZ, that the dose of organisms is small, but rather that the organisms in the infected lice are not usually in an infective phase. Unfortunately, I had no clinical material from which to obtain virulent blood to test, and see whether the monkeys which had been inoculated with cultures, and failed to react, were immune or not to experimental typhus.

SUMMARY.

1. The same coccus was found in the blood and urine of two patients suffering from typhus, and also in the urine of a convalescent of nine days' standing.

2. European typhus blood causes the same types of fever reaction in monkeys as American and North African typhus.

3. The coccus above mentioned has been isolated from the blood of infected monkeys.

4. Fever has been produced after a six days' incubation by the inoculation of a pure culture of the above coccus into two monkeys.

5. The above coccus is fairly closely related to cocci described by other authors, as far as the published data admit of a comparison.

6. The coccus is not injured by prolonged exposure to low temperature, viz., + 2° C.

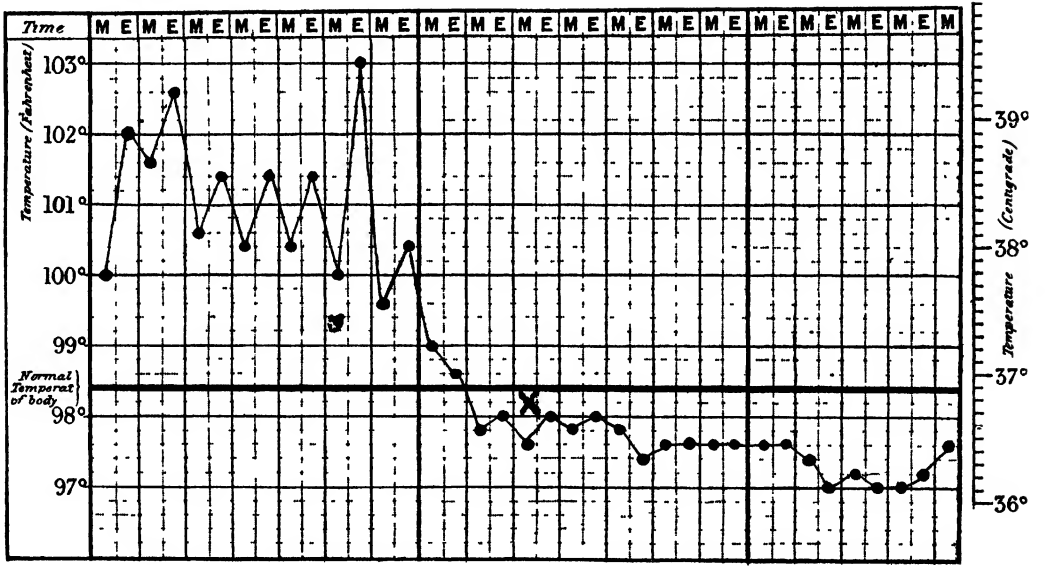
CONCLUSIONS.

1. The actual organism causing typhus is still not fully decided, but the balance of evidence available is in favour of its being due to a coccus.

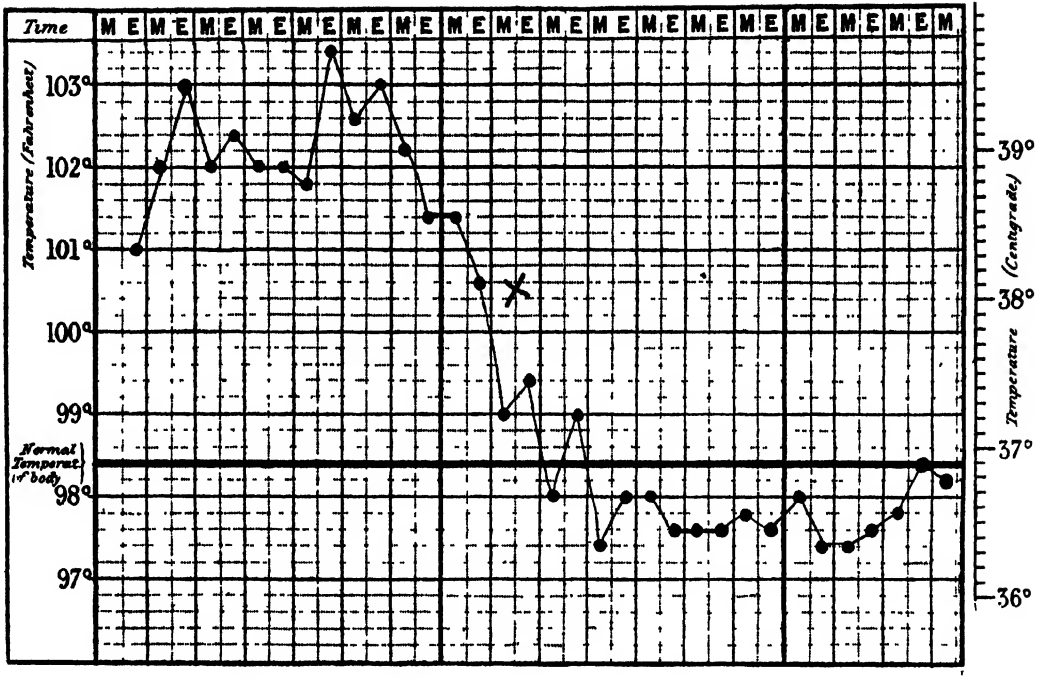
EXPLANATION OF SIGNS USED IN CHARTS.

X indicates when blood was taken from the patient or monkey.

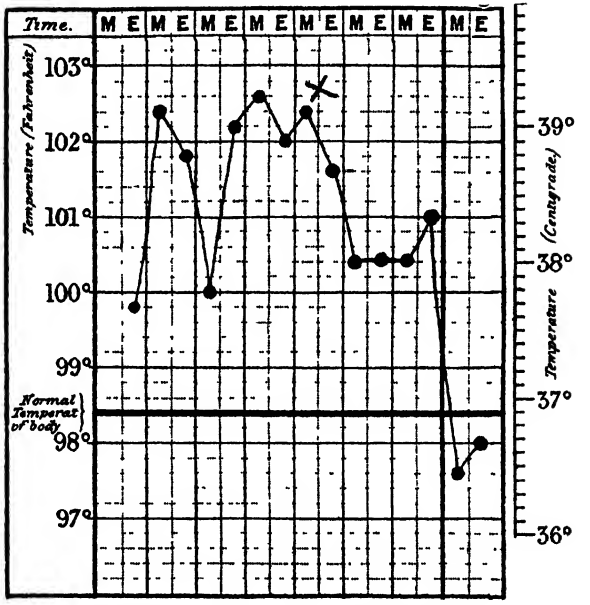
⊠ indicates when the blood or culture was injected into the monkey.



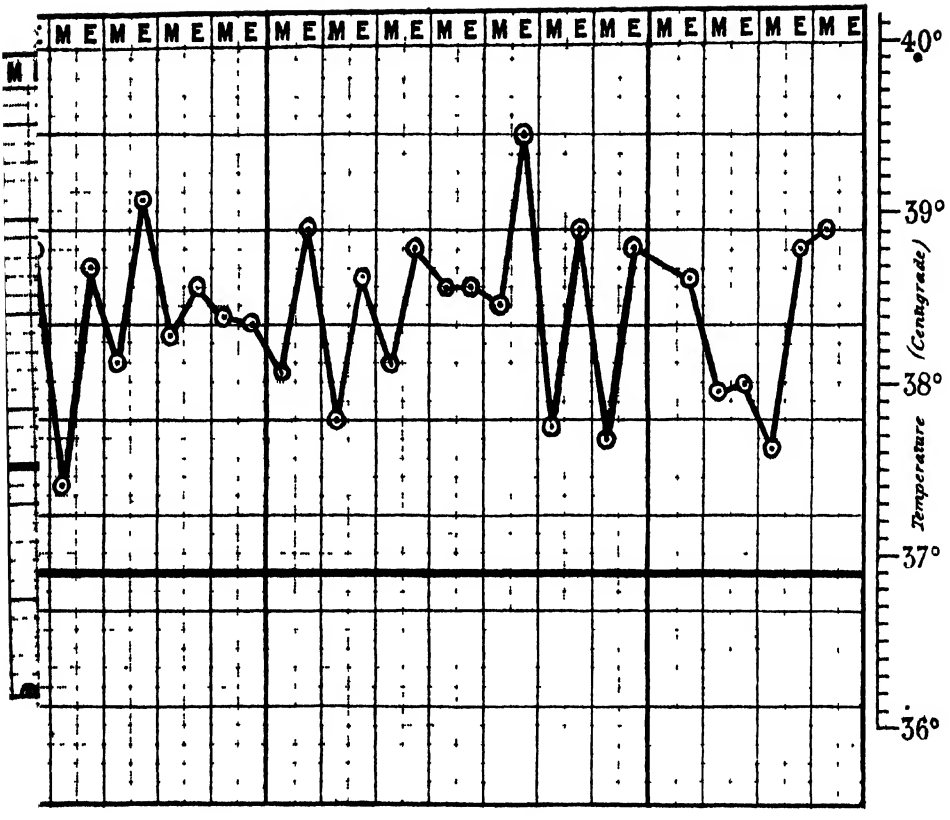
Temperature Chart of Patient No 1



Temperature Chart of Patient No. 3.



Temperature Chart of Patient No 4.


M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E M E
40°
39°
38°
37°
36°
Temperature (Centigrade)

2. The claims of PLOTZ to have discovered the cause of the disease are largely discounted by the contradictory nature of his publications.

3. Cocci have been found in the blood so frequently by independent and competent observers, that the use of these cocci tentatively as a prophylactic vaccine appears to be indicated.

DISCUSSION.

Fleet-Surgeon P. W. BASSETT-SMITH, in a letter, said: I am very sorry not to be able to be present this evening to hear Dr. PENFOLD'S interesting paper on a subject of such importance as typhus is at the present time. In reading it through, the advice given as to the undesirability of accepting PLOTZ'S organism and theory appears to be more than justified. From the experiments of NICOLLE and others, there is very little doubt of the infectiousness of the circulating blood of typhus from patients, and that the infecting agent can be, and generally is, carried by lice, and on that the main steps of prophylaxis must be founded. In his conclusion, the author states that the actual cause of the disease is not yet decided, it is, therefore, hardly logical or advisable to recommend the use of a vaccine made from a coccus, the relationship of which to the disease is very hypothetical.

Dr. G. C. LOW: I should like to express to Dr. PENFOLD how much we are indebted to him for giving us this paper to-night. I am sorry there is such a poor meeting, but, as Professor SIMPSON has already pointed out, the War has upset everything. We shall have the paper published shortly in the TRANSACTIONS, however, and those Fellows abroad, and others interested in the subject, will thus be able to profit by it.

I should like to ask Dr. PENFOLD if he has tried, or if he knows if filtering infective blood from typhus cases through filters, and then inoculating monkeys or people, has been tried? It may just be possible that the cause of the disease is a filterable virus, its spread by an insect host—though not proving this—being at least suggestive. I should like some information upon this if Dr. PENFOLD can give it me. Person ally, I cannot accept PLOTZ'S bacillus as the etiological factor of the

disease. Another point of interest is the question of Rocky Mountain spotted fever and its transmission by a tick. This disease has always been looked upon as a variety of typhus, and its transmission by such an insect is very interesting, especially so as ordinary typhus is spread by the louse. This would look as if the parasite, whatever it may be, will develop in different species of insects. Relapsing fever, a spirochætal disease, as is well known, is spread by lice also, and this might suggest a parasite of a similar or analogous nature, though so far, of course, none such has been found in typhus.

Professor W. J. SIMPSON: Speaking for myself, I may say that I think this is a very important paper, and I think Dr. PENFOLD is to be congratulated on the work he has done. He calls it a "preliminary communication," and consequently we do not expect very much in a note of this kind; and I hope that he will have a further opportunity, perhaps in the autumn, of carrying out this investigation to a further stage. It is an investigation attended with difficulties, and one of these is that of proving or establishing the specificity of the coccus. So far as I can gather, no small animals—laboratory animals—suffer from typhus, and the disease cannot be artificially produced in them. Therefore, one is at a disadvantage that does not exist in dealing with such diseases as tubercle, anthrax or plague. In these the micro-organism can be isolated from the patient, and will, by inoculation, produce a very definite disease in laboratory animals. It is a question whether the illness Dr. PENFOLD has produced in the monkey is a very definite disease. I think it would need a good many more experiments before one could say it was typhus fever. Monkeys are not easily obtainable, and they are expensive, which adds to the difficulty of experimental work of this kind; but probably once people were convinced that these experiments were valuable, the financial difficulty would be overcome. I think the reading of this paper to-day will shew the value of the experiments that have been made, and the need of more; and I hope the Military authorities, who are interested in this disease, will provide the funds and the opportunities for Dr. PENFOLD to go on with his researches.

In dealing with a subject like this, where no definite disease is produced in animals, there are certain pitfalls to be avoided. The criticism on PLOTZ's bacillus shews pretty plainly that it is very

questionable whether that bacillus is the cause of typhus fever. Mistakes of this kind are not at all uncommon, and accordingly it is necessary to be cautious. There is SANARELLI'S bacillus, which was at one time believed to be the cause of yellow fever; and then there was FREIRE'S cryptococcus, which was considered to be the infective agent of yellow fever, and a vaccine of which used to be employed for inoculation against the disease; then I remember the bacillus of malaria fever, until LAVERAN, MANSON and ROSS demonstrated that malaria was not a bacillary disease. So that if there is nothing very special about the features of a bacillus, and no definite disease is produced by it in the smaller animals, the difficulties to contend with are great. For some time I have been engaged on a research, in which I have endeavoured to isolate from slaughtered animals certain organisms of a particular kind, and, much to my confusion, although very carefully done, I have very often found a coccus in the blood of the animals I was dealing with. I believe that in a fair percentage of healthy animals micro-organisms can be cultivated from their blood. So much so, that I am beginning to wonder whether at times, if the blood of the healthy human being is taken, one would not sometimes find organisms in it, and much more so in those that are ill and weak, such as those suffering from typhus fever. I only mention this, not to depress Dr. PENFOLD, but merely as a warning to exercise caution on this subject; and I certainly agree with Fleet-Surgeon BASSETT-SMITH that it would be premature at the present moment to think of a vaccine. I should recommend Dr. PENFOLD, if possible, to get the opportunity I have mentioned, and to go on, and I wish him every success.

Dr. W. J. PENFOLD: First of all, in reply to Fleet-Surgeon BASSETT SMITH. He does not think it is logical to use a vaccine if it has not been demonstrated to be the cause. That is arguable. In disease we have not only a primary cause, but may have also secondary invaders. It may be a mistake to say that the body should not be protected against them. If we admit these are secondary invaders, I still hold that protection against them is indicated. WILSON found antibodies in the blood of typhus patients to the cocci he isolated, and the concentration of antibodies was five to twenty times that of the normal, so that the infected person immunises himself during the course of the disease.

On the question of infective filtrates which Dr. Low mentioned, that in the case of typhus we have to do with a filtrable virus is one of the legends grown up around typhus, and it depends on the work of NICOLLE. He did one experiment which appeared to shew that the virus passed through a filter. Different American workers, on the other hand, have done a large number of experiments, and they have not been able to get the virus to pass through a filter. The investigations of the American workers have been characterized by a large number of experiments and careful conclusions. NICOLLE and his co-workers no longer maintain that the virus is filtrable.

Rocky Mountain fever, I have little doubt, is typhus. If you immunise against Rocky Mountain fever, you immunise against typhus. The same thing applies to Mexican typhus (TABARDILLO).

In reply to the PRESIDENT, he dealt, first of all, with the specificity of the cause. These five strains of cocci were absolutely identical. I have never seen similar cocci arising as contaminations. If it were a question of casual organisms getting into the blood, which, I think, is quite possible, it would be very remarkable if the casual organisms were the same on five occasions. If, on the other hand, the organism had established itself in the blood, and grown regularly, my results would be accounted for.

I believe it is quite easy to make blood cultures regularly on human blood agar plates and get no growth. I think it can be done ten times as a sequence. In my case I took about ·2 c.c. of patients' blood, and I got a fair number of colonies, and all the colonies the same. I confidently believe, therefore, that this organism came out of the blood, and was not a casual contamination. Whether it is the cause or a regular secondary invader is another point.

The question of small animals is very important. If we could get a suitable small animal susceptible to typhus it would facilitate typhus work greatly. The guinea pig is now believed to be susceptible, though formerly this was denied. It is, however, more liable to sepsis, and other disturbances, and I think work involving temperature observation is at present best done on monkeys.

I agree that the most important thing to be done is to determine whether by immunising with the coccus we can produce immunity. If

we can inoculate the coccus into monkeys, and then shew they are immune to typhus blood, that is important.

I have not had access recently to typhus clinical material, but I am arranging to get an associate, and we are going to apply to the various Local Government Boards for material to work upon, in which case we hope to demonstrate if it is possible to immunise the body by means of this coccus against virulent patients'-blood.

Obituary.

Sir Francis Lovell, C.M.G.

Fellows will have learned, with great regret, of the death of Sir Francis Lovell, which took place in London on January 28th, 1916.

Sir Francis was an original Fellow of the Society of Tropical Medicine and Hygiene, which was founded in 1907, and took a keen interest in its work. In the last few years, however, a tendency to bronchial trouble prevented him going out at nights, so that he was unable to be present at its meetings. On his retirement from the Colonial Service in 1901, he became Dean of the London School of Tropical Medicine, and in this capacity visited the East to enlist sympathy for it, and at the same time to collect funds. He was strikingly successful, bringing back with him £10,000 or so in actual money and many promises of support. Later on he made another tour, this time to the West Indies, where he brought the name of the School prominently before the people of the different islands, and again collected a considerable sum of money. From that time till the present, Sir Francis lived principally in London, but as the years went on he found the winters becoming more and more trying, and often wished for the sunny climes where his early years were spent. Even till quite near the end, however, he still came to the School, and was always most interested in what was being done there. There is no doubt that his personality and kindly manner did much for that institution, and he was a great favourite with his colleagues as well as with the different generations of students who passed through the courses there. A writer in [illegible]'s obituary notice in the *British Medical Journal*, [illegible] well sums up his character:—"Sir Francis Lovell, [illegible]nd retiring, was a man of considerable force of character, [illegible] in the work of the School of Tropical Medicine, though [illegible]ntrusively exerted, was none the less active and beneficial. His [illegible]nd kindliness of manner made him everywhere popular,

and, while endearing him to those who knew him well, contributed greatly to the success of his missions."

This is very true, and the loss of Sir FRANCIS is a very real one, which it will not be easy to overcome. His name, however, and all he accomplished, will always be kindly remembered by his fellow-workers, as well as by Fellows of the Society who had the privilege of his acquaintance.

G. C. LOW.

LIBRARY NOTES.

Under this heading we propose to review recently published works sent us for that purpose, and a short notice of any other works presented to the library.

Sleeping Sickness.—Messrs. Baillière, Tindall & Cox have just published for the Centro Colonial, Lisbon, a work on sleeping sickness, this being a record of a four years' war against the disease in the island of Principe.* Originally published in the *Archivos de Hygiene e Pathologia Exoticas*, vol. v., March 30th, 1915, it has now been translated by permission of the Lisbon School of Tropical Medicine by Lieut.-Colonel J. A. WYLLIE, I.M.S. (retired), the authors being Drs. B. F. BRUTO DA COSTA, J. FIRMINO SANT' ANNA, A. CORREIA DOS SANTOS and M. G. DE ARAUJO ALVARES. The work is divided into five parts, as follows: (1) Main Facts Concerning the Epidemic—Natural Conditions of the Island; (2) The War against Sleeping Sickness in Principe—Plan of Sanitary Campaign and its execution; (3) Results of the Sanitary Campaign; (4) Sanitary Future of the Island; (5) Study of the Trypanosomiasis of the Island of Principe, etc.

The Islands of San Thomé and Principe in the olden days were free from tsetse flies. The former has remained so up to the present time, but unfortunately for the latter *Glossina palpalis* was introduced about the year 1825 and as it found conditions present there suitable it stayed and multiplied. Imported labour from the mainland supplied the other requisite, namely, the trypanosome, and since that time human trypanosomiasis has acted as a veritable scourge to the inhabitants of the island. The present work narrates how the Portuguese Government sent out different commissions to study sleeping sickness on the West Coast of Africa. The first of these, under the charge of Dr.

* ***Sleeping Sickness. A record of a four years' war against it in the island of Principe.*** **By B. F. BRUTO DA COSTA, J. F. SANT' ANNA, A. C. DOS SANTOS, and M. G. DE ARAUJO ALVARES. Translated by J. A. WYLLIE, F.R.G.S., Lieut.-Colonel Indian Army (retired). Baillière, Tindall & Cox, 8, Henrietta Street, Covent Garden, London. 1916. 7s. 6d. net.**

BETTENCOURT, paid a passing visit to Principe in May, 1901. The second mission, under the charge of Dr. MENDES, remained in the island throughout the years 1907 and 1908, and started the essential elements for the sanitary campaign which has just been satisfactorily concluded by the third and present mission.

The experiment is a most interesting and instructive one. Here we have a tropical island covered with dense vegetation and virgin forests, conditions which would seem to render the stamping out of the fly, distributed practically all over the area, impossible. During the observations on the subject it was found that the fly lived in close proximity with the vast herds of wild pigs, descendants of domestic pigs which had been allowed to spread over and perpetuate themselves throughout the island. Wherever pigs were found, so also tsetse flies *(Glossina palpalis)* abounded. It was at once seen that, in addition to stamping out the *Glossina*, all these pigs and other wild animals—monkeys and civet cats—would have to go. Amidst such difficulties then, the mission tackled the subject in earnest and with scientific acumen, year by year, clearing all the undergrowth, draining the swamps, and so gradually lessening the number of tsetse flies, until, in the year 1914, they were able to announce the complete disappearance of *Glossina palpalis* from the island, and the extermination of the epidemic of sleeping sickness. Such a splendid result is on a par with the extermination of tropical diseases in the Canal Zone, and the stamping out of malaria in parts of the Malay Peninsula. It shews what really can be accomplished in the way of tropical sanitation when the subject is approached energetically and in earnest. Of course, an island is a specially suitable place for conducting such a campaign, and that must be borne in mind, but it is clear that even if similar methods are employed on the mainland *Glossina palpalis* can be stamped out in suitable areas, and sleeping sickness therefore gradually diminished or abolished. Unfortunately in this instance wild animals, which may act as reservoir hosts, cannot be so completely exterminated as were the pigs on the island of Principe, but still much may be accomplished. There is one thing certain, and that is that the good work of draining and clearing which the different missions have carried out in Principe should not now be allowed to fall into abeyance; a chance tsetse fly might quite easily be introduced again, and all the labour and trouble would have to be repeated. It is to be hoped,

therefore, that the work will be kept up, and a very careful search should be conducted from time to time to make certain that no *Glossina* have crept back again or that no new ones have been introduced from without.

In order thoroughly to appreciate the value of the work done and what actually has taken place, one must, of course, read the report in detail. The splendid illustrations, taken from photographs, which are so abundantly interspersed throughout the pages of the work are most instructive, and shew at a glance, both the nature of the tropical vegetation which had to be dealt with, and how this has actually been accomplished and carried out. The work is a splendid pioneer example of prophylaxis against the tsetse fly—the carrier of the trypanosome of sleeping sickness—and should be widely distributed in the British and other colonies where this disease is prevalent.

In addition to the sanitary part of the report, the last chapter is devoted to the study of the form of the different trypanosomes met with in the island, with notes on the hæmatophagous insects and intestinal flagellates of *Glossina palpalis*. These are of importance and distinctly valuable.

Finally, a word of congratulation upon the skilful way in which Lieut.-Colonel J. A. WYLLIE has translated the Portuguese text. This could not have been improved upon. The fact of having the work produced in English will add enormously to its value, as it can now be widely distributed and read. It should act as a basis for all other tsetse fly campaigns which may have to be carried out in the future.

G. C. LOW.

Pseudotphoid Fever in Deli, Sumatra (a variety of Japanese Kedani Fever), by WILHELM SCHÜFFNER.—In the *Philippine Journal of Science* (Vol. 10, Sec. B, No 5, September, 1915), SCHÜFFNER contributes a very interesting paper on a disease seen in Sumatra, which, though resembling enteric fever in its general clinical course, appears to be distinct, and to have affinities with kedani fever as seen in Japan. Until recently this latter disease has only been known in Japan, but in 1908 ASHBURN and CRAIG described an analogous disease in the Philippine Islands, and it is probable that it occurs in other countries as well. In Japan the disease

only occurs at certain times of the year, which are determined by the periodical floods. In Sumatra there is no such regularity, the disease being observed throughout the year. A second important difference is in regard to mortality. In Japan it is accepted that an average mortality of about 30 per cent. occurs, whereas in Sumatra this only amounts to 3 per cent. There is also a difference in the transmitting agents of the disease in the two countries. In Japan a small, red mite, the larval form of an unknown *Trombidium*, is known to be the infecting agent, the true host of this parasite being the field mouse, which harbours the parasites often in large numbers about the ears. Up to the present, SCHÜFFNER has been unable to determine the transmitting agent of the disease in Deli, but here also, judging by the histories of the patients, it is a question of ticks or mites. On the estates where the disease occurs the labourers suffer greatly from the attacks of minute acarines, red in colour, and so small as to be scarcely visible to the naked eye, Professor NUTTALL, who examined these, has found them to be of two kinds, one the larval form of a *Trombidium*, and resembling the kedani mite, the other a larval form of *Cheyletus*. It has not been possible to determine the species in either case, as the adults are unknown. As regards the symptoms, in 39 per cent. of the cases the original point of infection is discoverable; while in Europeans it is seen in all cases. In the earliest stage the lesion shews itself as a flat vesicle, 3 to 4 c.mm. in diameter, surrounded by a dull red areola. The papule soon bursts and beneath it there appears a small dark area of blackish necrosed skin some 4 c.mm. in diameter; five to eight days later the slough is cast off, leaving a small round or oval ulcer with steep edges and the floor covered with mucopus. The ulcer is of an indolent character, shews small tendency to healing and may persist throughout the illness. Lymphangitis has not been observed, but the lymphatic glands in the neighbourhood of the ulcer are enlarged and tender, sometimes markedly so; in some cases they may even reach the size of a pigeon's egg. The site of the initial lesion varies, but is commonest in the regions of the groins, the armpits, and the neck. As the lesion is minute it is frequently only recognised with difficulty. The second characteristic symptom of the disease is an eruption which appears on the second or third day of the illness and attains its full development on the sixth to the eighth day; it then presents itself as a roseola, the raised spots varying in size from

that of a hemp seed to a threepenny bit. The rash covers most of the body, being thickly placed on the flanks and less marked on the face and extremities. It closely resembles the roseola of secondary syphilis and persists from eight to ten days, then changes to a brownish colour and slowly disappears. It may not be easily visible on the dark skin of the native.

The course of the fever can best be described by saying that it corresponds in all respects to that seen in enteric fever. In severe cases the temperature attains its maximum in four to six days, remains high for some time, and then gradually falls by lysis. This distinguishes it from typhus, with its quick onset and termination by crisis. In some cases the fever shews a remittent type, in others it remains high for ten days, then falls, and then rises again for about the same time. The nervous system may be effected, much in the same way as in real typhoid, restlessness, violent headaches and drowsiness then being seen.

The leucocytes are increased in number from 10,000 up to 12,000 per cubic millimetre. Differentially there is a large relative increase of lymphocytes, which may be due to the general involvement of the lymphatic glands met with in the disease.

There are no characteristic symptoms associated with the internal organs. Diarrhœa is uncommon. Albuminuria may be present, while extensive bronchia-pneumonia has been seen in fatal cases. Rheumatoid pains in the smaller joints are common. Convalescence is slightly protracted. The patient's serum does not react to *B. typhosus*, *B. paratyphosus A*, and *B paratyphosus B*; attempts to cultivate organisms from the blood also failed. It was not found possible to infect monkeys by injection of the blood from cases of the disease. Postmortem, only the lesions seen in ordinary pyrexia were present. SCHÜFFNER summarises the subject as follows:—

1. There exists in Sumatra a disease which resembles enteric fever in its general clinical characters, but is clearly distinct from that disease in causation.

2. There is evidence that this disease is transmitted in a manner similar to that which has been demonstrated for kedani fever in Japan.

3. Though the pseudotyphoid of Deli would appear to be a much

less fatal disease than kedani fever of Japan, there are yet many points of resemblance between the two diseases.

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G. C. LOW.

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THE SANITATION OF A SMALL EUROPEAN SETTLEMENT IN PORTUGUESE EAST AFRICA; WITH NOTES ON SOME OF THE DISEASES PREVALENT IN THE DISTRICT.*

BY

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In April, 1913, I was sent by the Church of Scotland Blantyre Mission as one of a party to open up a new station in Portuguese East Africa, to the east of the Mission's present stations in British Nyasaland.

This paper is primarily an account of the measures taken for the sanitation of the new station, but the investigations recorded, undertaken in this connection, may be worthy of record as having a bearing on the distribution of some of the diseases prevalent in the district.

GEOGRAPHICAL. METEOROLOGICAL.

The station is situated about 160 miles east of the Nyasaland border, and about 30 miles south of the 15th south parallel of latitude, in the

*A thesis presented to the University of Glasgow for the degree of M.D., 1915, for which the degree, with commendation, was granted.

Portuguese administrative district of Alto Mólôcuê. It has an altitude of about 2,800 feet above sea level, the elevation of the country generally being lower than that of British Nyasaland, and the temperature consequently higher. The following table gives the monthly maximum and minimum, and average temperatures, and the monthly rainfall during a year of our residence. For these figures I am indebted to the Rev. JAMES REID, F.R.S.G.S., who has kept continuous records since the establishment of the station.

TABLE I.

CHURCH OF SCOTLAND, ALTO MÓLÔCUÊ.

TABLE OF TEMPERATURES AND RAINFALL.

Month.	Temperatures.			Rainfall (in inches).
	Highest Shade Temperature. Degrees Fahr.	Lowest Shade Temperature. Degrees Fahr.	Average for Month. Degrees Fahr.	
1914.				
February	84	71	75·7	7·97
March	82	67	74·3	10·35*
April	91	60	74·7	3·14
May	83	59	69·2	0·64
June	88	54	68 7	0·26
July	83	53	65·1	1·11
August...	91	57	72·5	0·29
September	94·5	60	80·3	0·09
October	96	63	81·1	0·01
November	94	58	77·7	5·05
December	97	68	81·5	6·49
1915.				
January	94	69	80·2	7·58

*Heavy storm on the 5th. Rain gauge overflowed.

POPULATION.

The natives are of the Bantu stock, and belong to the Alomwe tribe, who occupy a large portion of Portuguese East Africa north of the

Zambesi River. They are subdivided into smaller sections, such as Mihavani, Esherima, Makuwa, and others. The people in our immediate neighbourhood call themselves Esherima. They live in villages, which may be two to ten miles apart, each village consisting of from twenty to one hundred or more huts. The huts are very scattered, and the larger villages may occupy nearly a square mile of country. From the medical point of view the natives make good patients, when their confidence has once been won, but they have an instinctive distrust of European treatment. It was some months after my arrival here before the people in the surrounding villages could be persuaded to come to me for treatment.

The Europeans on the station numbered seven, including two children. The administration officials, who are few and scattered, are the only other Europeans in the district.

So far as I am aware, no systematic medical work has ever been done in this part of Portuguese East Africa before. A Portuguese Sleeping Sickness Commission visited the district in 1912, but up to the time of writing this I have been unable to obtain a copy of their report. Statements in text books of tropical medicine as to the existence of particular diseases in Portuguese East Africa, generally refer to the area south of the Zambesi, or to the European settlements on the coast. The district of Alto Mólôcuê was first occupied by the Portuguese administration so recently as six years ago.

MALARIAL FEVER.

In the selection of the site for the new station, my first consideration was, naturally, the freedom of the locality from the breeding places of mosquitoes. This consideration, however, has always to be made compatible, as far as possible, with a good water supply at a convenient distance, and a situation suitable for a vegetable garden. As there is practically no rainfall for six months of the year, this latter desideratum necessitates a soil more or less permanently damp, or sufficient water for irrigation purposes. A reference to the attached map at the end of the paper will shew the relation of the dwelling houses to the position of the garden and the well from which the water supply is drawn.

The station is situated on the top of a broad ridge, about 200 feet above the level of the surrounding country, and the wells and garden are

in a narrow valley about 100 feet lower than the houses. This valley contains several springs and some marshy ground, forming the source of a small stream, which runs permanently during the rains but is partially dry up to a mile below the station from August on to the end of December.

The two other stream beds, marked (1) and (2) on the map, were dry at the time the site was settled on (end of June), and no other possible sources of anopheline breeding grounds were found within a mile radius of the station, nor have since been found. Beyond that radius, however, and up to a distance of two miles, several such breeding places exist. The nearest village is about two and a half miles away.

A well was dug for the Europeans' water supply on the opposite slope of the valley from the houses on the station, and another was made farther down the stream for the use of the natives resident on the station. Two pools were left for the purpose of irrigating the garden. The rest of the swampy ground was then drained, and the stream bed cleaned out, defined, and straightened.

Mosquitoes were never numerous; they were, indeed, much less so than in most of the Mission's Nyasaland stations, where primary malarial infection is not common. The majority of the mosquitoes present were anophelines; culicines were very rare. Nevertheless, several cases of malaria occurred, four of them undoubtedly fresh infections. Two of the patients were children (one European and one native) who had been born on the station and had never been away from it. The first two cases occurred in January, 1914, and there were three cases in April. One case occurred in May, and one in June, both in native children, but these two latter were possibly relapses of old infections. In all these seven cases the simple tertian parasite was found in the blood.

My register of dispensary patients shews that a corresponding rise took place in the number of malarial patients from the villages, at the same time of the year; there was a noticeable increase in January and the following months, with a much larger rise in April, after which the attendances for malaria gradually fell. This year, 1915, a similar increase has occurred among the dispensary patients, in January.

A reference to the rainfall table will shew that the beginning of the increase in malaria takes place in the third month of the rains. It is to

be noted, however, that it is not till January that the rains begin to have any effect on the swampy ground and the streams, and consequently on the breeding places of anophelines. December is the driest month of the year for streams and marshy ground. The greatest incidence in malaria will be seen to coincide with the end of the rains, and the following month. In the Mission this was the time of year when anophelines were most numerous, and an explanation of this was afforded by the nature of their breeding grounds found in the neighbourhood.

The dry stream beds marked (1) and (2) frequently contained pools during the rains, but examinations of these very rarely shewed anopheline larvæ, though culicines were often found in the dirtier pools. In April, however, numerous anopheline larvæ were found in them, and I discovered that some of the pools persisted during May and part of June, though the upper reaches of the streams were dry. During January and February a few anopheline larvæ were found in places where the danger of their being washed away by the flush of rain did not exist, namely, in the Europeans' well and at the sides of the small drainage channels in the marshy ground below the garden, and these, doubtless, accounted for the few anopheline mosquitoes present at that time.

A few of the anophelines found in the district are being sent to Professor NEWSTEAD, F.R.S., of Liverpool University, who has kindly identified blood-sucking flies for me on a former occasion. One of the species being sent appears from the data I have at hand to be *Myzomyia funesta*. This mosquito was shewn by DANIELS to be the chief carrier of malaria in British Central Africa, and it is, therefore, likely to be the carrier here. It is a typical spring and fresh water breeder,[1] and the collections of water in the neighbourhood of the station in which anopheline larvæ have been found are almost wholly of the type which would precisely suit its requirements.

As to the human element in the chain of infection, that was always present in the natives who lived on the station. I took, on one occasion, specimens of the blood of the twenty-nine natives, children and adults, who are permanently on the station, and found malarial parasites in six of these. Parasites had also been found in several others during attacks of fever at various times. This permanent reservoir is liable to be supplemented at intervals by the natives' friends from the villages

coming to stay with them. If these friends are women they are almost inevitably accompanied by young children, who, as will be shewn later, are a nearly unfailing source of infection.

PREVENTIVE MEASURES.

From the time that the first cases of fever occurred, regular attention was paid to the sources of mosquitoes. The pools in the stream beds were filled up with earth and stones, and the drainage channels in the marsh were deepened and cleaned, or increased in number, according as best prevented the stagnation of water. Where the fall of water in the channels was not sufficient to prevent stagnation, these were swept down at regular intervals. Grass was kept short in the marshy area, so that fresh collections of water could be readily detected. The irrigation pools in the garden were kept free from weed, and the sides clean-cut so as to avoid shallow water at the edges. Such pools have been found by FRY, in India, to be less favourable to the breeding of mosquitoes than those with shallow margins and weed in which the surface-living anopheline larvæ can take refuge from their natural enemies.[2] Most of these measures had to be repeated at intervals during the rains, for fresh collections of water appeared in the marsh as the season proceeded, and each accession of heavy rain altered the character of the channels, washing out pools that had been filled up and forming fresh pools, often in new places.

When the rains were over, a final draining of all the pools left generally sufficed until the beginning of the next wet season. In some cases, however, I found that water which flowed freely at the end of the rains, became stagnant as the marsh or stream dried up, necessitating some fresh measures of drainage at various periods during the dry season. In some of these, the necessities of the case were met by filling up the channel altogether. Oiling of pools was never found practicable. Owing to heavy carriage and import duties, petroleum is too expensive for such use here. My experience, moreover, shews that in small pools the oil is washed out by the first shower of rain.

The Europeans' well is now being fitted with a pump, and when this has been done it will be completely closed in. The pools on the rocks, and the clay pits, shewn on the map, in which culicines were found

breeding, were filled up, and gave no further trouble, as there was no wash of water to scour them out again.

It was formerly considered that half a mile was beyond the normal flight of anophelines,[3] and one regarded breeding grounds outside that radius as having no influence on the malaria of a locality. Experiments in the Panama Canal Zone with dyed mosquitoes, however, have shewn that some species, at least, can travel a mile.[4] JAMES, in his mosquito work in Ceylon, has also noted in Colombo migrations of great numbers of mosquitoes whose nearest breeding grounds were nearly a mile away.[5] Other similar observations have been made. The possibility of such long flights, therefore, would have to be taken into consideration in the event of persistence of anophelines or malaria after all the nearer breeding places had been dealt with. It may be mentioned that since June, 1914, up to the present time (March, 1915), no cases of fresh infection have occurred on the station.

As has been noted, the natives living on the station, especially children, formed the source from which the infection was drawn by the mosquitoes. In the circumstances of the new station, this source, unfortunately, could not be avoided. A reference to the map will shew how closely the houses are surrounded by forest. Owing to official delays in our obtaining a lease of the land, we were unable to make a larger clearing, and as leopards were troublesome at the beginning of our occupation, the native staff were naturally unwilling to live farther away in the uncleared bush. When the station is permanently settled, houses for the native staff, especially where there are children concerned, will be built at a distance away from the European dwellings. It is essential, however, to have a certain number of natives living on the station. House-boys are indispensable to the European, and their dormitory must be near his house. Among the house-boys on the station, I have found malarial parasites on at least four occasions during attacks of fever. A certain amount of risk from this source is thus inevitable. The ordinary labourers formed a settlement on the banks of a stream about a mile away from the station,

The administration of quinine to patients was persisted in as long as possible, as much as a general prophylactic measure as for the treatment of the individual case. In Europeans quinine was continued for two months. In natives great difficulty is experienced whenever they feel

well, and a certain amount of compulsion or moral suasion, as best meets the case, has to be applied.

Mosquito nets were made the rule for Europeans. This precaution was specially liable to be neglected in circumstances like ours, where noisy and troublesome culicines are rare, and the silent, inconspicuous anophelines, biting mostly through the night, are apt not to be noticed. The keeping of bedroom doors and windows open at night more than compensates for the "stuffiness" of the mosquito net, and a covering of chicken netting on doorways and windows gives sufficient protection from wild animals and other possible dangers.

A testimony to the value of the mosquito net was incidentally afforded. On one occasion one of the European families were driven out of their bedroom in the middle of the night by an invasion of red ants, and had to spend the rest of the night in another room on improvised beds, without nets. Twelve days later, on the same day, two of the family went down with an attack of malaria, the parasite being recovered from the blood in both cases.

INVESTIGATION OF MALARIA OF THE DISTRICT.

In order to get an idea of the extent and nature of the malaria of the district, I examined 84 children from the nearest village (2½ miles from the station), representing the majority of the juvenile population. The spleen was palpated and blood slides made in each case. The details of the investigation are shewn in Table II, in the Appendix. The ages of the children ranged from 1½ months to 14 years, but the majority were between 1 and 6 years old.

Malarial parasites were found in the blood of *72* children, *i.e.*, *85·7* per cent. of the total number.

Enlarged (*i.e.*, palpable) *Spleen* was found in *61* children, *i.e.*, *72·6* per cent. of the total.

Positive Evidence of Malarial Infection (*i.e.*, parasites or enlarged spleen or pigment in the mononuclear leucocytes) was found in *79* children, *i.e.*, *94·04* per cent. of the total.

Among the 72 positive blood slides:—

29, or *40·2* per cent. shewed *Malignant Tertian Parasite (Laverania malariæ)*.

43, or *59·7* per cent., shewed *Simple Tertian Parasite (Plasmodium vivax)*.

6, or *8·3* per cent., shewed *Quartan Parasite (Plasmodium malariæ)*.

In four cases the species of the parasite was uncertain. There were ten cases of mixed infection—seven cases of malignant with simple tertian, one case of malignant tertian with quartan, and two cases of simple tertian with quartan. Gametocytes were found in six cases.

The endemic index of the district is thus seen to be very high, and the liability of the European immigrant to infection in the villages will be very great. Mwanyewe's is a typical village, and has no special features which are not possessed by all native villages to predispose it to a high malarial endemicity.

A relative leucocyte count was made in the case of the five children who shewed no other signs of malaria. The results are shewn in the following Table. Five hundred leucocytes were counted in each case.

TABLE IIA.

RELATIVE LEUCOCYTE COUNT OF CHILDREN SHEWING NO OTHER SIGNS OF MALARIA.

Number of Case.	Polymorphs.	Large mononuclears.	Lymphocytes.	Transitional.	Eosinophiles.	Basophiles.	Myelocytes.
32	15·8	52·6	16·0	13·8	1·6	0·2	—
35	50·4	26 4	17·0	4·6	1·2	0·4	—
37	40·4	42·4	8·6	3·4	5·2	—	—
52	47·4	32·0	3·6	3·2	13·4	0·4	—
55	22·8	50·0	19·6	5·2	1·8	0·2	0·4

It will be seen that in all the cases the percentage of large mononuclears is exceptionally high. A relative increase in the mononuclears has been noted in other protozoal diseases, and CASTELLANI and CHALMERS state that as a means of diagnosis in malaria a marked increase may arouse suspicions, but that by itself it is quite useless.[6] In the case of these native children, however, the increase is almost certainly due to malarial infection. In all the other cases a similar mononuclear increase could be noted on a cursory examination of the blood, and malaria is the only protozoal infection to which village children are universally liable. When the percentage infected is so high, it is unlikely that any should escape. One of the children on this list of five (No. 55) was brought to the dispensary six weeks later, suffering from a heavy infection of the malignant tertian parasite.

Examination of the blood for parasites is seen to reveal a higher percentage of malaria than palpation of the spleen. It is to be noted, however, that the investigation was made in January, when illness from fever begins to be common in the villages. If made in August or September, the positive findings from examination of the blood would probably not have been so numerous.

The higher percentage of the simple tertian parasite, as compared with the subtertian (malignant) form, is an interesting point. The Malaria Commission of the Royal Society to British Central Africa, in 1899, found only the malignant tertian parasite in the Blantyre district. The simple tertian form is now also found there, but it is not common. From the foregoing investigation, as well as from my experience of malaria on the station and among dispensary patients, the simple tertian form would appear to be the common one here. As, however, *Laverania malariæ* completes its schizogony in the internal organs, it is probable that the findings of the peripheral blood do not give a true index of its frequency.

One of the most remarkable points brought out by the examination, is the very large numbers of parasites which these native children are able, through the gradual development of immunity, to harbour in their blood without apparent symptoms. In one or two cases, parasites were present to the extent of two or three to every field of the microscope, and one to every two or three fields was fairly common. In Europeans,

in Central Africa, such large numbers are very uncommon in my experience. Symptoms in them are produced by much smaller numbers, and quinine treatment checks multiplication before it proceeds much further. It should be noted, nevertheless, that most native children suffer from malaria, from time to time, and the mortality among them from the malignant form is probably great.

From the large numbers of parasites which they are able to tolerate without untoward effects, one may draw the practical conclusion that in native children malaria is not necessarily the primary cause of any illness accompanied by fever, even though the malarial parasite may be demonstrated in the blood. As an illustration, I recall such a case in a child treated by me for malaria who returned on the following day with a commencing smallpox eruption.

INTESTINAL PARASITES.

Soon after beginning medical work among the natives here, I found that ankylostomiasis existed, and, knowing that the infection was fairly common in Nyasaland, I was anxious to find out to what extent it was present here. For this purpose I examined the fæces of 200 ordinary natives of the district. These were drawn mainly from the raw labour on the station, supplemented by house-boys and other permanent employees, dispensary ulcer patients and any villagers whom I could get hold of. I wished to get at least 500 cases, but having no hospital with patients in residence I found great difficulty in obtaining specimens, even with the offer of a small reward of calico for each contribution. The presence of other helminths was noted and tabulated at the same time, and the details of the investigation are shewn in Table III, in the Appendix. The figures in each column represent the number of ova found under a circular cover-glass, $\frac{7}{8}$in. in diameter; the maximum quantity of fæces which when mixed with water allowed sufficient clearness for the ova to be distinctly detected, being taken in each case. A rough estimate of the relative heaviness of each infection was thus made.

The results of the examination are shewn in the following Table:—

TABLE IV.

Results of Examination of Fæces of Ordinary Population, Alto Mólôcuê.

Parasite.	Number Examined.	Ova found in.	Percentage infected.
Ankylostoma duodenale or *Necator americanus* .	200	194	97·0
Ascaris lumbricoides	200	128	64·0
Schistosoma (?) *mansoni*	200	4	2·0
Trichuris trichiura (trichocephalus) . .	200	29	14·5
Tænia sp. (?)	200	10	5·0

ANKYLOSTOMIASIS.

Ova of Ankylostoma or Necator were found in 194 cases, or 97 per cent. The number investigated is too small to enable one to speak definitely for the whole population over a large area, but it may be safely said that the percentage of the population infected is very large. The cases represented most of the villages within a radius of twenty miles of the station and many others beyond, up to a distance of fifty miles. Several women, with a number of children down to three years of age, were included in the investigation, and all of these gave positive findings. I examined independently a few children of two years of age and under, but found no ova.

For comparison with these findings I may quote the results of an investigation by Dr. Stannus, of the Nyasaland Government Medical Service.[7] In 505 Nyasaland natives examined by him in Zomba (mostly prisoners and native troops), 22·1 per cent. were found infected. Stannus quotes Bass as having shewn that by the method of examination of fæces for ova 20 cases per cent. were missed, and he, therefore, arrived at the figure of 42 as representing the probable true percentage of infected in Nyasaland.

In several of the cases of my own series, which gave negative results on the first examination, I had an opportunity of examining a second

specimen, and all of these except one gave positive findings—sometimes only after examining a second slide. It may well be, therefore, that the percentage of infected in this district is as high as 100.

Regarding the degree of infection, an estimate of the number of worms present may be attempted from the number of ova found. As quoted by STANNUS, BASS estimates that one egg per microscopic slide indicates an infection with less than ten worms; and NICOL, in India, gives it as his opinion that to produce effects on the host there must be 500 worms present for six months, though other authorities put this number as low as 50.[8] The largest number of ova per slide in my series was 38. The great majority contained less than 12. Many slides shewed only 1 ovum. The average number in the positive cases, 5·8 per slide. According to the estimates of BASS and NICOL, therefore, none of these cases were heavily enough infected to be prejudicially affected by the parasite.

With a view to finding out the approximate number of worms present in these cases, as well as for the purpose of ascertaining the species of parasite, I examined the stools of several cases whom I had treated. In the absence of a hospital I found great difficulties in the treatment. It was nearly impossible to persuade the patients to abstain from solid food on the day before the administration of the drug, and I found that, in the presence of the enormous masses of material which the intestines of these vegetable feeders contain, the drug had almost no effect on the parasite. Others of my cases went off to their homes before I had obtained the stools which they passed after treatment, and, finally, being unable to keep the cases under my care, I was unable to give such large doses as otherwise I could have done. The drug used was thymol, usually in three 30-grain doses for adults. Such doses are easily borne by the natives.

In ten of the cases examined, the number of worms found varied from 1 to 138, 14 or 15 being the usual number. In all these cases, however, ova were still found in the stools a week after treatment, though in diminished numbers. Case 63, who followed my instructions more intelligently than any of the others, may be taken to form the basis of a calculation. His stools shewed the unusually large number of 33 ova per slide, and 138 worms were found after treatment. A week later the fæces still shewed three ova to a slide, but he was unwilling to undergo

further treatment. Making allowance for some worms which may have been missed in the search, as well as for some which may have undergone digestion, owing to the gradual poisoning with the drug and slow passage through the bowel, as suggested by BOZZOLO,[9] one may reckon that 200 worms were accounted for by the treatment. Calculating from the numbers of the ova found before and after treatment, this number of 200 represents ten-elevenths of the total number of worms, and one, therefore, arrives at 220 as the total number originally present. This number accords with BASS'S estimate as to the relationship of the number of ova to the number of worms, and this is borne out also by my findings in other cases.

As to the pathological effects, this case, as has been noted, shewed a degree of infection unusually large in my series. In his case, as in one or two others of the more highly infected, a degree of anæmia was present, recognisable by pallor of mucous membrane: no hæmic cardiac murmurs were found in any case examined. In children and young adults a certain amount of any anæmia present must be put down to malaria. A degree of eosinophilia, up to 12 per cent. in some cases (*cf.* Table IIa Case 52, p. 137), is common among natives here; in many, however, this is due, in part at least, to the presence of *Ascaris lumbricoides*, as it is most marked where this helminth is present. I have, however, seen several patients (shewing ankylostome ova) who complained of abdominal and general symptoms, which were probably referable to the ankylostome infection, and where the parasite is so widespread it is to be expected that a certain proportion of the population will be so heavily infected with the worm as to suffer from its effects. The large majority, however, must be regarded as merely carriers; a condition which obtains in other parts of Africa, as well as in India and other countries where the worm is endemic.

The existence of a relative racial immunity has been shewn by the Porto Rico Commission, who found that though Europeans, mulattoes and negroes were infected to nearly an equal degree, the percentage with severe symptoms was much the greatest in the first race and was least in the last.[10]

It is highly probable that such an immunity exists among the natives of Central Africa, where, presumably, the disease has existed for many generations.

SPECIES OF WORM.

Both *Ankylostoma duodenale* and *Necator americanus* were found in the stools examined. The latter seems to be the commoner parasite, as in most of the stools it alone was present. In two cases a third species of Strongylidæ was found, which appears to conform to the description of *Triodontophorus deminutus*. All the material collected is being taken to Dr. LEIPER, of the London School of Tropical Medicine, for examination.

The large percentage of the population affected, together with the comparatively slight degree of infection in each case, accords well with the sanitary habits of the natives. Most of the villages are built close to a small stream or some marshy ground which forms their water supply, and close to which they may grow their green crops in the dry season. As the grass in such places is long, they constitute the natural latrines of the village, for no attempt whatever at sanitation is ever made. The ankylostome larvæ are thus provided with all the requisites of temperature and moisture for their development and continued existence, especially during the rains. Numerous paths traverse these marshy spots, and, assuming the skin to be the usual site of invasion the natives are probably infected either on these paths or during defæcation in the bush, or while working in the moist soil of their wet gardens. Thus opportunities for infection, while obtaining equally for all natives, are not constant in any one case, nor, owing to the scattered position of the houses, is the liability to a heavy infection great. The duration of the life of the ankylostome and necator in the intestine has not been determined, but the longest period stated by MANSON for the ankylostome is three years.[11] As the natives harbour the parasite throughout life from childhood onwards, it is obvious that re-infection must take place from time to time.

The higher percentage of infection here, as compared with Nyasaland, may be due to the higher temperature at this lower altitude favouring the development of the larvæ. Another probable factor, which bears also upon the prevalence of ascariasis, is the undoubtedly more careless habits of the Alomwe in the matter of defæcation, feeding, etc., as compared with those of some of the Nyasaland tribes. On account of their dirty habits the Alomwe are universally looked down upon by these latter, who frequently refuse to eat out of the same dish with them, or

even out of dishes which have been handled by them. The conditions round about the drinking wells in the villages are sometimes remarkably filthy.

In the present condition of the country, where villages and houses are so scattered, the influence of ankylostomiasis upon the general health of the population is probably not great. The disease, however, would be very liable to assume some economic importance, were the country to be opened up to civilisation, and agricultural or other industries to develop, which would cause the collection of large numbers of the population as labour into restricted areas without adequate sanitary provision. All the conditions for massive as well as constant infection would then be present. Such has been the history of ankylostomiasis in Porto Rico and other West Indian islands.

ASCARIS LUMBRICOIDES.

In the 200 cases examined, 128 shewed ova of ascaris, a percentage of 64. The contrast from the conditions in Nyasaland is striking. In STANNUS'S series of 505 cases, not a single ovum of ascarsis was found.[7] During four years in Blantyre, I have examined several hundreds of stools, and have not seen ascaris ova in more than 10 or 12 of these. In the present series nearly all the villages shewed cases, but some villages appear to be more heavily infected than others. Children and adults seem to be affected equally. The largest number of ova per slide found was 245, but the number of ova seems to depend upon the size as well as on the number of the worms present. Many cases were treated: among those which I had the opportunity of following up, the largest number of worms expelled from a single case was 15.

TRICHURIS TRICHIURA.

Ova of trichuris were found in 29 cases—14·5 per cent. Ova were always scarce—never more than four to a slide. It is thus probable that cases of infection were missed, and that the percentage infected is higher than represented by the positive findings.

TÆNIA.

Ova were found in 10, or 5 per cent. The species of tape-worm was not ascertained, as I did not have an opportunity of treating any of these cases.

The percentage found does not probably represent the full incidence of infection of the population, as proglottides were not specially searched for in the fæces, only the onchospheres lying free being noted.

SCHISTOSOMA.

Ova were found in four cases. In two of these (No. 8 and No. 33) the ova were lateral spined. In the other two (No. 34 and No. 158), the one ovum found in each case was terminal spined. Cases No. 8 and No. 33 both gave a history of having incurred the infection outside the district, and the former was found to be also suffering from the urinary form of the disease which, he stated, began just after the rectal symptoms had passed off. Among many ova found in his urine, one was lateral spined. Case No. 34 gave no history of dysenteric symptoms, but stated he had suffered from hæmaturia as a child. His urine contained ova, all those seen being terminal spined. I did not have an opportunity of enquiring into the history of case No. 158.

From my experience of dispensary patients, urinary schistosomiasis appears to be fairly common in the district. It is seen in children who have never been away from their villages. I have not, however, come across any locally contracted cases of the rectal form of the disease. Both forms are very common around Blantyre, where I have once seen a terminal spined ovum from the rectum, and once also a lateral spined ovum from the bladder.

OXYURIS VERMICULARIS.

The thread worm appears to be of common occurrence in the district. In the examination of fæces for *Ankylostoma*, I have come across it in small numbers; in 9 out of 11 cases examined, though microscopically, I have encountered its ova in only one case of the 200.

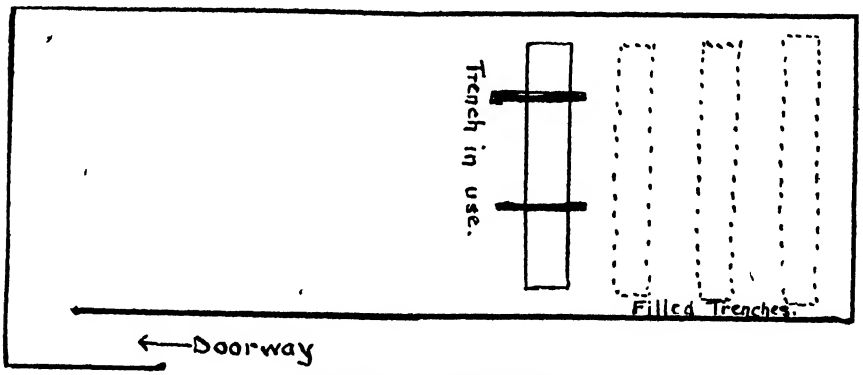
STRONGYLOIDES STERCORALIS.

In the microscopic examination of stools in Blantyre district, the larva of this parasite is frequently met with. It may be worthy of record that it was not once seen in the examination of the 200 stools in this district.

PROPHYLACTIC MEASURES.

Since the settlement of the Mission, from 40 to 60 natives have been constantly employed as raw labour, and about 30 of a permanent staff besides are always on the station. These numbers are likely to be increased in the near future, and brick-making and other similar operations will be undertaken, in which the workers will be in constant contact with mud, possibly infected with ankylostome larvæ. The universal prevalence of ankylostomiasis made it necessary, therefore, to arrange the latrine accommodation with reference to the mode of spread of this disease; the frequency of the *Ascaris lumbricoides* being also borne in mind. In our circumstances nothing elaborate or expensive could be attempted, but the necessities of the case were, I think, sufficiently well met. The deep trench system was adopted, and the plan of the latrine is shewn in the diagram.

Walls are of grass, and the buildings are roofless. Trenches are made about six feet deep, the first trench being dug at the end farthest



PLAN OF LATRINE.

from the doorway. To facilitate defæcation into the trench, wooden cross pieces, on which the native can stand, are laid over the trench. A native is employed to go round the latrines daily and cover in the fæces to a depth of about six inches. As each trench gets filled up a fresh one is dug nearer the doorway, and thus contact of the natives' feet with contaminated earth of the old trenches is avoided. When all the space becomes used up, a new latrine is built in a fresh dry locality.

In the dry season the absence of a roof has everything in its favour. Everything in the trench is baked thoroughly dry by the warm sun,

making conditions impossible for the existence of the larvæ. Even during the rains the advantages—apart from the expense—are in favour of a roofless latrine in suitable soil. Intervals of several days of bright sun, without rain, are frequent, when the superficial soil becomes thoroughly dry. There are several hours of sunshine almost daily throughout, and experiments by JOYEUX, in Upper Guinea, shew that exposure of *Necator americanus* larvæ to two-and-a-half hours of tropical sunlight (? sunshine) kills them.[12] The effect of exposure to sunshine upon pathogenic bacteria, which may be present in the fæces, must also be taken into consideration. A certain amount of mild compulsion has to be imposed on the native to make him use the latrines, and in this connection an enclosure open to the sky is more attractive to him than a roofed building would be.

In the conditions under which the European lives in tropical Africa, the probabilities of infection with ankylostomiasis are not great. Cases, however, have occurred in Nyasaland,[13] and those whose work may expose them to contact with contaminated earth should be warned of the possibilities. Children especially should not go about without shoes during the wet season.

The general hygienic measures adopted on the station, apart from ankylostomiasis and malaria, may be mentioned briefly. For European latrines the bucket system with earth was adopted; buckets being emptied daily into a pit at a distance and the discharges covered in.

All water for drinking and cooking purposes is boiled. My experience of filters, apart from the question of their failure to arrest certain organisms, is, that in the hands of native servants they are very liable to get damaged, and householders are apt to rely upon water as safe which has passed through an inefficient and perhaps dirty filter.

Instructions were given that all fruit and vegetables eaten uncooked should be washed in boiled water. In the disposition of latrines, etc., everything possible was done to prevent the possibility of contamination of the garden area with fæcal matter.

For the prevention of flies, no special measures were taken beyond the ordinary regular sweeping of the station, the burning of refuse and rubbish during the dry weather, and the burying in pits of what could not be burned. Hitherto, flies have not been troublesome.

A lookout is kept for empty tins and bottles, such as might breed

culicine mosquitoes. *Stegomyia calopus* is occasionally seen, but I have found that it breeds in collections of water in the hollow stumps of trees in the forest.

Two other tropical diseases may be briefly referred to before concluding this paper.

SLEEPING SICKNESS AND TRYPANOSOMIASIS.

As previously mentioned, I have unfortunately been unable to obtain a copy of the report of the Portuguese Sleeping Sickness Commission which visited this district among others in 1912, but I met the principal member of the Commission in Blantyre before coming up here, and he informed me that he had not found sleeping sickness in Alto Mólôcuê district, and that tsetse fly of all species were very scarce, though in Baixo Mólôcuê, the administrative district lower down the Mólôcuê River, the fly was widely distributed. Major DO REGO, the chief administrative official of Alto Mólôcuê, has informed me that tsetse fly exists a little to the south of the administrative station, which stands at an elevation of 2,000 feet about 30 miles south of the Mission, and that all his cattle have died from the bite of the fly. I have not come across any species of tsetse in the neighbourhood of the Mission.

Sir DAVID BRUCE, from his experience of *Trypanosoma rhodesiense* in Nyasaland, is convinced that this trypanosome is identical with *T. brucei*, the pathogenic agent of the bovine disease nagana.[14] If, as is probable, the species of tsetse fly present in Alto and Baixo Mólôcuê is *Glossina morsitans* the possibility of human infection with *T. brucei* or *rhodesiense*, as in Nyasaland, will exist, though the common infective agent for animals carried by this fly may be *T. pecorum*.

RELAPSING FEVER (AFRICAN TICK FEVER).

During my residence here I have come across one case of human spirochætosis. Inquiries indicated, however, that the disease was contracted at Quilimane, on the coast, where it is known to be endemic. I have found the spirochæte in the blood of several Nyasaland carriers who had just returned from Quilimane, where, they stated, they had been bitten by the tick, and were severely ill on the return journey back. These men belonged to districts in Nyasaland in which the disease is not endemic.

I have made enquiries among the people of this district, but none of them appear to know the tick or the disease caused by it. In the districts of Nyasaland in which tick fever exists, both the disease and its cause are well known by the natives. From these considerations one may infer that the disease is not endemic here. I have not encountered *Ornithorodoros moubata* in any of my journeys among the villages.

CONCLUSIONS.

In so far as the work recorded in this paper lends itself to the forming of conclusions, these may be summed up as follows :—

Malaria.— The endemic index of the district is over 90, and is probably 100. The commonest parasite appears to be *Plasmodium vivax*, the simple tertian form. Native children may harbour large numbers of parasites in their blood without symptoms.

Ankylostomiasis.—At least 97 per cent. of the population harbour *Ankylostoma duodenale* or *Necator americanus*. The latter appears to be the commoner parasite. The majority of the population are merely carriers.

Ascariasis.—Over 50 per cent. of the population harbour *Ascaris lumbricoides*—adults and children alike.

Trichuris trichiura.—This worm is not nearly so common as the preceding one. It is present in small numbers in about 14 per cent. of the population.

Schistosomiasis.—The urinary form of this disease is common, but the rectal form has not been found, locally contracted.

Tænia.—Tape worms exist, but their frequency and species have not been accurately determined.

Oxyuris vermicularis.—This parasite is probably widely distributed, at least in small numbers.

Spirochætosis.—African tick fever is probably not endemic in the district.

Trypanosomiasis.—Sleeping sickness is not known to exist in the Alto Mólôcuê district. Tsetse fly is sparsely distributed.

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TABLE II.

Examination of Native Children for Malaria, Mwanyewe's Village.

No. of Case.	Sex.	Age.	Spleen.	Malarial Parasites.			Malarial Pigment in Leucocytes.
				Subtertian (Malignant)	Simple Tertian.	Quartan.	
1	F.	10 years ..	Not palpable	—	3	—	—
2	F.	3 months ...	2 inches below C.M....	20	—	—	—
3	F.	4 ,, ...	Not palpable	—	3	—	—
4	M.	4 years ...	2 inches below C.M....	40	—	—	—
5	M.	2 ,, ...	Just palpable	-	-	—	—
6	F.	1 year ...	2 inches below C.M....	—	—	—	—
7	M.	3 years ..	1 inch ,, ..	4	—	—	—
8	M.	3 ,, ...	3 inches ,, ..	2*	—	—	x
9	F.	3 ,, ...	Not palpable	5	10	—	—
10	F.	3 ,, ...	Just ,, ...	—	10	—	—
11	M.	1½ ,, ...	2 inches below C.M....	—	—	—	x
12	F.	4 ,, ...	2 ,, ,, ...	1†	—	—	—
13	F.	4½ ,, ...	2 ,, ,, ...	—	—	4	—
14	F.	14 ,, ...	3 ,, ,, ...	1	—	—	—
15	M.	2½ ,, ..	1 inch ,, .	-	100x	—	-
16	M.	3 ,, ...	Just palpable	20	2	—	—
17	M.	6 ,, ...	3 inches below C.M....	10	20	—	—
18	M.	2½ ,, ...	1 inch ,, ...	2	—	—	—
19	F.	2 ,, ...	1 ,, ,, .	15	—	—	—
20	M.	1½ ,, ..	Not palpable	4	—	—	x
21	M.	1 year ...	,, ,, ...	1*	-	—	—
22	F.	9 months ..	,, ,, ...	5	—	—	-
23	F.	1 year .	1 inch below C.M. ...		2	-	x
24	F.	2 years ...	1 ,, ,, ...	70	-	—	—
25	F.	2 ,, ...	Not palpable	—	1	—	—
26	M.	3 ,, ...	,, ,, ...	-	12	—	—
27	M.	4 ,, ...	3 inches below C.M....	150†	—	—	—
28	M.	1½ ,, ..	Just palpable	—	1	—	—
29	F.	2 ,, ...	2 inches below C.M....	15	—	—	—
30	F.	1½ ,, ...	2 ,, ,, ...	30	—	—	x
31	M.	5 ,, ...	½ inch ,, ..	60	—	—	-
32	M.	1 year ...	Not palpable	—	—	—	—
33	F.	2 years ...	,, ,, ...	10	5	—	—
34	M.	1½ ,, ...	3 inches below C.M....	—	—	15	—
35	F.	6 months ...	Not palpable	- -	—	—	—
36	F.	1 year .	2 inches below C.M....	1*	—	20	—
37	F.	9 months ..	Not palpable	—	—	—	—
38	F.	5 years ...	,, ,, ...	10	10	—	—
39	F.	10 ,, ...	,, ,, ...	5	—	—	—
40	F.	9 ,, ...	1 inch below C.M. ...	—	5	—	—
41	F.	9 ,, ...	2 inches ,, ..	5	—	—	—
42	M.	4 ,, ..	3 ,, ,, ...	20	—	—	—
43	F.	6 ,, ...	1 inch ,, ...	—	5	—	—
44	F.	1 year ...	Just palpable	—	10	—	—
45	M.	5 years ...	2 inches below C.M. ..	—	10	—	—

TABLE II.—*continued.*

Examination of Native Children for Malaria, Mwanyewe's Village.

No. of Case.	Sex.	Age.	Spleen.	Malarial Parasites.			Malarial Pigment in Leucocytes.
				Subtertian (Malignant)	Simple Tertian.	Quartan.	
46	M.	5 years	4 inches below C.M....	2		-	—
47	F.	6 months ...	2 ,, ,, ..	—	15	—	—
48	M.	9 years ..	Not palpable ...	—	3*	—	-
49	F.	2 ,, ...	,, ,, ...		3	—	—
50	M.	6 ,, ..	2 inches below C.M..	-	2	-	—
51	M.	4 ,, ...	1½ ,, ,, ..	--	2	2	--
52	M.	12 ,, .	Not palpable .	-		-	
53	M.	2½ ,, ...	2 inches below C.M...		2*		x
54	M.	6 ,, ...	Not palpable	2		-	x
55	M.	4 months ...	,, ,, ...	-	-	-	—
56	F.	2 years ..	2 inches below C.M. .	20	20	-	—
57	M.	5 ,, .	1 inch below C.M. .				-
58	F.	6 ,, ...	Not palpable	2†	- -	—	-
59	F.	10 ,, .	2 inches below C.M. ..	—	100	—	—
60	M.	1½ ,, ...	Just palpable	—	2		—
61	F.	2 ,, ..	,, ,, ...	--			—
62	F.	1 year ...	1 inch below C.M. ..	--	--	-	---
63	M.	5 years .	2 inches ,, .	-	1	1	-
64	M.	2 months ..	1 inch ,, ..		1		—
65	M.	2 ,, .	1 ,, ,, ...		3		—
66	F.	3 ,, ...	1½ inches ,, ..		20	-	
67	M.	6 years ..	1 inch ,, .	--	3	—	
68	F.	6 ,, .	1½ ,, ,, ..	-	6	-	—
69	F.	3½ ,, ...	Not palpable	—	3		-
70	F.	3 ,, .	2 inches below C.M....		—	20	—
71	F.	5 ,, ...	3 ,, ,, ..	3	-	—	-
72	M.	1½ months ..	Not palpable .. .		2	—	
73	M.	6 years ..	1½ inches below C.M...	-	2	—	—
74	M.	6 ,, ...	1 inch ,, ...	—	20	—	—
75	M.	12 ,, ...	1 ,, ,, ...		80	—	—
76	F.	13 ,, ...	Just palpable		—	-	—
77	M.	6 ,, ..	,, ,, ...	—	1	—	—
78	M.	2 ,,	2½ inches below C.M...	10	25	—	—
79	M.	2 ,, ...	2 ,, ,, ...	—	30	—	—
80	M.	3 ,, ...	3 ,, ,, ..	—	8*	—	—
81	M.	1 year ...	1 inch ,, ...	-	120	—	—
82	M.	2 years ...	Not palpable	109	—	—	x
83	M.	1 year ...	1 inch below C.M. ...	—	40	—	—
84	M.	2 years ...	1 ,, ,, ...	—	5	—	—

* Gametocytes found in these cases. † Species of parasite uncertain.

NOTE.—The numbers in the various columns indicate the approximate number of parasites found in a twenty minutes' search.

TABLE III.

Examination of Fæces of Native Population, Alto Mólôcuê District

No. of Case	Number of Ova per Slide					No. of Case	Number of Ova per Slide				
	Ankylostome	Ascaris	Schistosome	Trichuris	Tænia		Ankylostome	Ascaris	Schistosome	Trichuris	Tænia
1	4	—		1	8	45	10			2	—
2	11		—			46	1	4		1	
3	5	—	—		—	47	9		—		
4	7	29			—	48			—		
5	5	73		—		49	2				—
6		35				50	1				
7	2					51	13				
8	3	—	1			52	7				
9	10	—				53	14	106	—		
10	1					54	17	42	—		
11	14					55	16	4			
12	8	30				56	2	7	—		—
13	1	3				57	5		—	—	
14	4	3			—	58	8	52		2	—
15	3	5		1	—	59	12	174	—		
16		10				60	3	—			
17	18	61				61	12	126		—	—
18	7					62	5	4		—	
19	1			—		63	35	33		—	
20	5	3	—	—	—	64	9	37		—	
21	2	16	—			65	2	17			—
22	1	—				66	2	36		2	—
23	5	15	—		—	67	7	15			
24	2	—	—	—	—	68	5	126	—	1	26
25	6	10	—	—	—	69	2	39	—	—	—
26	3	18	—	—	—	70	9	64	—	1	—
27	4	—	—		—	71	1	—	—	—	—
28	4	51	—	3	—	72	16	—	—	—	—
29	2	24	—	—	—	73	3	52	—	—	—
30	2	—	—	—	—	74	5	22	—	—	—
31	2	—	—	—	—	75	1	—	—	—	—
32	3	95	—	—	—	76	12	34	—	—	—
33	9	73	3	—	—	77	10	27	—	—	14
34	14	129	1	—	—	78	21	67	—	—	—
35	—	—	—	—	—	79	14	245	—	—	—
36	7	59	—	1	—	80	6	—	—	—	—
37	2	44	—	—	—	81	20	174	—	—	—
38	8	81	—	—	—	82	17	104	—	—	—
39	1	9	—	—	—	83	5	37	—	1	—
40	2	—	—	—	—	84	3	12	—	—	—
41	—	—	—	—	—	85	2	17	—	—	—
42	2	—	—	—	—	86	1	37	—	—	—
43	7	15	—	—	—	87	1	—	—	—	—
44	2	40	—	—	—	88	9	—	—	—	—

TABLE III.—*continued.*

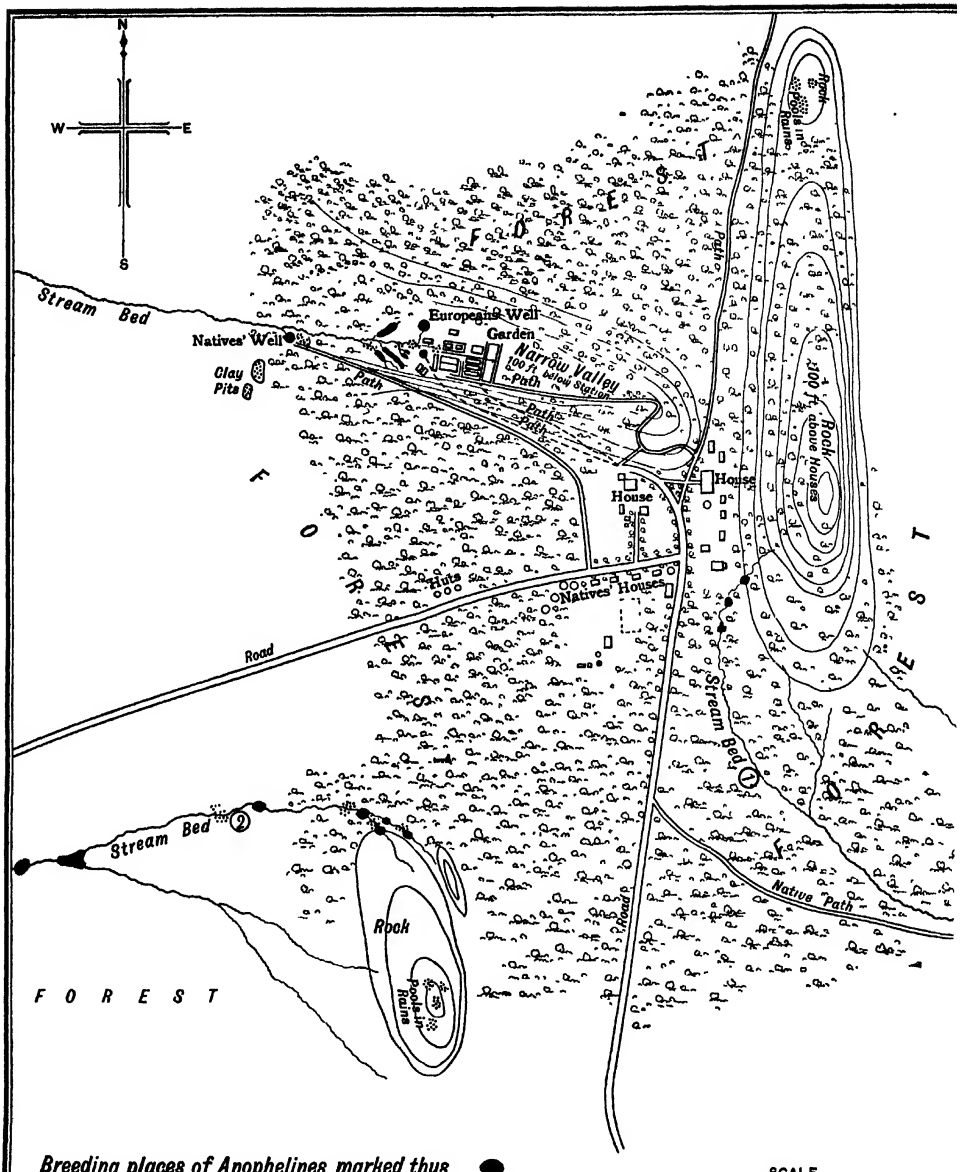
Examination of Fæces of Native Population, Alto Mólôcuê District.

No. of Case.	Number of Ova per Slide.					No. of Case.	Number of Ova per Slide.				
	Ankylostome.	Ascaris.	Schistosome.	Trichuris.	Tænia.		Ankylostome.	Ascaris.	Schistosome.	Trichuris.	Tænia.
89	5	—	—	-	—	133	3	—	—	—	—
90	5	11	—	—	—	134	2	12	—	—	—
91	3	31	—	2	—	135	2	48	-	-	-
92	8	12	—	1	—	136	22	—	—	—	9
93	1	12	-	—	—	137	3	-	-	1	—
94	7	67	—	1	—	138	3	77	—	1	—
95	7	3	-	-	—	139	8	5		—	—
96	38	55	—		—	140	1	3	-	—	—
97	12	5	—	—	—	141	4	- -	—		—
98	1	-	-	—	-	142	8	78	—	—	—
99	4	70	—	—	—	143	4	13	-		—
100	18	—	—	- -	—	144	6	15	-	—	—
101	1	—	—	—		145	6	52	-	1	—
102	12	35	—		—	146		151	-		—
103	6	56	—	—	—	147	3	39	—	1	-
104	14	37	—	—	1	148	2	—	—	—	—
105	6	—	-			149	1	8	-	—	—
106	3	46	-	—	—	150	5	6	—	-	—
107	3	—	—	—	—	151	6	—	—	—	—
108	5	32	—			152	1				-
109	1	29	—		—	153	7	6		—	2
110	14			-	—	154	1	4	—	—	—
111	8	12		-	—	155	9	155	—	—	—
112	19	3		—	—	156	4	—	—	- -	- -
113	6	—	-	—		157	3	49	-	—	—
114	1	8	—		-	158	3	2	1	—	—
115	1	—	—		1	159	8	7	—	—	9
116	6	—	—	—	—	160	1	—	—		—
117	9	127	—	—	-	161	5	2	—	—	—
118	5	39	—	2	—	162	1	—	—	—	—
119	1	—	-	—	—	163	8	31	—	4	—
120	8	11	—	—	—	164	5	48	—	- -	—
121	2	16	-	—	—	165	4	65	—	—	—
122	1	—	—	1	1	166	7	60	—	—	- -
123	3	—	—	—	—	167	1	14	—	—	—
124	1	14	—	—	—	168	5	—	—	—	—
125	1	—	—	—	-	169	3	9	—	—	—
126	7	24	—	—	—	170	7	4	—	—	—
127	3	—	—	—	—	171	15	—	—	1	—
128	1	48	—	—	—	172	8	—	—	1	—
129	10	145	—	2	—	173	1	—	—	—	—
130	6	—	—	—	—	174	1	—	—	—	—
131	2	96	—	—	—	175	2	—	—	—	—
132	1	40	—	—	—	176	1	12	—	—	—

TABLE III.—*continued.*

Examination of Fæces of Native Population, Alto Mólôcuê District.

No. of Case.	Number of Ova per Slide.					No. of Case.	Number of Ova per Slide.				
	Ankylostome.	Ascaris.	Schistosome.	Trichuris.	Tænia.		Ankylostome.	Ascaris.	Schistosome.	Trichuris.	Tænia.
177	7	—	—	—	—	189	2	—	—	—	—
178	17	9	—	—	—	190	1	—	—	1	—
179	7	67	—	1	—	191	4	—	—	—	—
180	7	—	—	—	—	192	2	6	—	—	9
181	1	—	—	—	—	193	2	33	—	—	—
182	1	16	—	—	—	194	1	72	—	1	—
183	4	35	—	—	—	195	3	12	—	—	—
184	5	66	—	—	—	196	1	4	—	—	—
185	9	78	—	—	—	197	1	8	—	—	—
186	4	—	—	1	—	198	3	74	—	—	—
187	2	22	—	—	—	199	8	59	—	—	—
188	15	6	—	—	—	200	7	80	—	—	—
				Total Positive Cases			194	128	4	29	10
				Percentage Infected			97	64	2	14·5	5



MAP OF MISSION STATION, ALTO MÓLÔCUÊ.

To show Mosquito Breeding Grounds, etc.

LIBRARY NOTES.

Under this heading we propose to review recently published works sent us for that purpose, and a short notice of any other works presented to the Library.

Rural Sanitation in the Tropics, being notes and observations in the Malay Archipelago, Panama and other Lands. By MALCOLM WATSON, M.D., C.M., D.P.H. With Illustrations. London: John Murray, Albemarle Street, W. 1915. Price, 12s. net.

Dr. WATSON has rendered a distinct service to tropical medicine by writing the above book. It is a record of the most important work that has been done in tropical sanitation, as regards the destruction of mosquitoes, since the time these insects were incriminated as the carriers and disseminators of malaria and yellow fever, and it takes us up to the present day.

In the introductory chapter, the author states how his unavoidable isolation from fellow-workers was a source of constant anxiety to him, the fear ever being present that his views might become narrow and fixed from constantly seeing the same surroundings, and that his ideas would be altered had he more knowledge of what was going on and being done in other places. These thoughts drove him to visit other lands to see how they were dealing with sanitary problems, and the results of his own work in Malaya, and what he saw and thought of the work of others, are now recorded in the present pages. The book is dedicated to Sir RONALD ROSS, whose epoch-making discovery of the *rôle* played by the mosquito in the propagation of malaria has led to great sanitary activity and scientific investigation of disease throughout the tropics. There are eighteen chapters, as follows:—(1) Introductory; (2 and 3) British Malaya; (4) Quinine Prophylaxis in Italy; (5) Malaria in India; (6) Notes on Sumatra; (7) Hong Kong and the Philippine Islands; (8, 9, 10, 11, 12, 13, 14, 15 and 16) Panama; (17) British Guiana; (18) Barbados; and, finally, an epilogue entitled The Place of Sanitation in Tropical Colonisation. A large part of the book, it will thus be seen, is devoted to

Panama, the Mecca of the modern sanitarian, according to the author. This includes a detailed account of the sanitary organisation, and tells how one of the largest labour forces that the world has seen has not only been built up, but also maintained at a high degree of efficiency, in one of the deadliest climates in the tropics when engaged on a great engineering work.

Dr. WATSON'S own work in Malaya—work of the very greatest importance—comes first. This was of a pioneer nature, and the author had to decide between one or other of the three following methods, viz., mechanical protection, quinine prophylaxis, or mosquito reduction. He began his experiment in a small town called Klang, and, to suit the local conditions, he determined that any expenditure should be on works of a permanent nature, such as draining and filling up swamps. The drainage of the town was to be looked upon as a test case, and it proved to be sound in every respect, as in a couple of years Dr. WATSON was able to report that malaria had ceased to be of any practical consequence. Port Swettenham was next experimented upon, and the results of the operations there were equally satisfactory. Further studies of adjoining districts led to the belief that the reduction of malaria to a negligible quantity was quite possible, and for this agricultural development was what was required; the disappearance of stagnant pools—the breeding haunts of *A. umbrosus*—being followed by a cessation of malaria. Though this applied to the flat coastal zone, it was quickly found, however, that it did not to the hill estates, so a careful examination was carried out in these areas. This resulted in an anopheline with different habits *(A. maculatus)* being detected as the carrier of the disease—an insect which breeds in clear springs and crystal brooks, and cannot be eradicated by weeding the edges of the streams. Different measures had to be adopted, therefore, to meet the case, and, finally, an ingenious system of pipe drains was developed.

How Kuala Lumpor, the Federal Capital, was also freed from malaria by Mr. EVANS, Engineer to the Malaria Advisory Board, is also described, and some useful rules for Estate Sanitation are also given here.

Passing over the chapters on Italy and India, that on Sumatra is reached, and this forms very instructive reading. The Dutch have taken up the matter of the prevention of malaria and other tropical diseases in great detail, with excellent results. Their hospitals, according to

Dr. WATSON, are of the best; they have a splendid Medical Institute, and their estates are in a first-rate condition, with little or no malaria present. Their work is indicative of energy and progression, and similar steps might well be followed in other colonies.

The chapters on Panama must be read to be appreciated. It is a wonderful record, and one wonders what would have happened if President Roosevelt had sent a "practical man" instead of one who wasted time and money in fighting mosquitoes. The success of the work is largely due to him for not only having supported Colonel GORGAS, but also for having raised him to a seat on the Canal Commission. Unless this had been done, sanitation would have been hampered, and the work would never have been carried out. How near failure it really was may be appreciated if one reads page 115 of Dr. WATSON'S book. When 62 cases were reported in June, 1905, a panic only was averted with the greatest difficulty. The overcoming of this outbreak was indeed a triumph for Colonel GORGAS.

Turning finally to the chapters on British Guiana and Barbados, it is clear to those who know those areas that a more prolonged visit by Dr. WATSON might have modified his views considerably. It seems somewhat optimistic to say that in this part (Corantyne Coast) of British Guiana agriculture has in some way, for all practical purposes, extinguished malaria. Though perhaps not so malarious as the Malay Peninsula, yet British Guiana cannot be considered a health resort in this respect. Dr. WATSON says on page 274 that in a closed trench next the hospital no larvæ were found. This shews the danger of generalising on one observation; the present reviewer used to use this same trench for the collection of anopheline larvæ for experimental purposes many years ago.

As regards Barbados, there are other collections of water which Dr. WATSON did not see, and in which mosquito larvæ lived. The writer of this note went into the subject of the absence of anophelines from Barbados, in detail in 1900, and Dr. WATSON quotes some of those results now. Experiments carried out with "millions" in artificial collections of water, shewed that these fish did feed greedily on mosquito larvæ and pupæ, but that in natural collections, even though present in large numbers, larvæ of different culex mosquitoes (*Melaconion atratus*, etc.), nevertheless occurred. Such being the case, it did not seem necessary

to reply to the letter of Mr. GIBBONS in 1905, as this author had evidently not studied the subject in detail, and his observations were therefore valueless. Though fish play a part in keeping down the numbers of mosquitoes, they cannot, or at least do not, exterminate them in natural collections of water.

An epilogue, entitled The Place of Sanitation in Tropical Colonisation, closes this very excellent work. Here the sanitarian is invited to take a broad view of life, and is further counselled to strive to realize what the difference between surplus and deficit really is; and to remember that, since sanitation is primarily a question of ways and means, he must cut his coat according to his cloth. These are wise words, and future sanitarians would do well to note them carefully.

Dr. WATSON'S work then is an excellent one, and many first-rate photographs help to enhance its value. All interested of course in tropical sanitation must read it. It is hoped that these few notes put together here will direct the attention of workers in the tropics to it and whet their appetite to peruse and study its pages themselves.

G. C. Low.

TRANSACTIONS
OF THE
SOCIETY OF TROPICAL MEDICINE
AND HYGIENE.

APRIL, 1916.
VOLUME IX. No. 6.

Proceedings of a Meeting of the Society held on Friday, March 17th, 1916, at 5.30 p.m., at 11, Chandos Street, Cavendish Square, W., Mr. J. CANTLIE, in the Chair.

EXPERIENCES IN THE TREATMENT OF SYPHILIS IN THE ARMY, WITH SPECIAL REFERENCE TO THE ADMINISTRATION OF "606" IN CONCENTRATED SOLUTIONS.

BY
CAPTAIN H. J. McGRIGOR, R.A.M.C.,
Specialist in Dermatology, Aldershot Command.

These experiences in the treatment of syphilis may be presented in the form of a report on the work done at the Connaught Hospital, Aldershot, from 6th September, 1914, to 31st December, 1915.

The cases treated in the venereal division were drawn from the whole of the Aldershot Command, and also from the Front, with occasional transfers from other Commands.

During the period above mentioned, there were treated:—

Syphilis (new admissions)	1084
,, (transfers)	89
Gonorrhœa	3096
Venereal Sores	124
Skin and Scabies cases	2907

Regarding the cases of syphilis, it will be seen from the above figures that they are practically all from the troops actually in the Aldershot Command. Among those admitted under heading "new admissions" were a number of cases of what we call "old syphilis." These we consider to be "old cases" where the disease had been acquired more than twelve months prior to admission.

They represented 17·7 per cent. of the total new cases.

The "old cases" comprised many old infections in men who had previously been soldiers, and who had received a considerable amount of treatment in past years, while in the regular Army. Most of these men declared that they had been free of all symptoms while in civil life, but that the stress and strain of training caused them to break down again.

On making enquiries of these men as to the health of their families, we elicited the statements from most of them that during their years of civil life they had married, and their families were healthy. Occasionally one would admit that abortions had occurred, but the most held to the statements about healthy children having been born to them.

The transfers from other Commands amounted to about 8 per cent.

The districts of the country in which the infections were acquired was carefully inquired into, and it was found that the disease had been got in practically all parts of the United Kingdom and Ireland, Canada, France, India, etc. The largest number of cases came from the following towns: London, Dublin, Glasgow, Newcastle. Of these, London was the greatest focus of infection.

It is interesting to note here that the cases of infection acquired in the Aldershot Command represented only 4·2 per cent. of the total admissions for syphilis.

All cases of syphilis when diagnosed were treated in hospital and kept in hospital until they were free from active manifestations, such as unhealed sores on the genitals, condylomata at anus, mucous patches in the mouth and throat. The average stay in hospital to accomplish this was 15·5 days.

On leaving hospital, all patients had to attend weekly for their mercurial and other injections, according to the scheme of treatment in vogue at the time.

During this period of treatment inside hospital and also as out-patients, careful attention is paid to the general condition of the patients

as regards weight, appearance of the gums, any onset of anæmia, and the care of the teeth. The urine is also examined periodically.

The sites of the primary lesions in this series of cases were in the following order of frequency:—

1. Most common: Sulcus of penis, *i.e.*, at junction of the foreskin mucous membrane with that of body of penis.
2. Next most common: On the glans penis itself.
3. Least common: On the body of penis.
4. Rarest: Extra-genital chancres on finger, lip, tongue, eyelid, anus.

The lesions which were noted in the diagnosis of the cases occurred in their relative frequency as noted just below, and, of course, their sequence depended on the length of time since infection and whether any treatment had been given.

1. Hunterian chancre.
2. Glands—enlargement and shottiness in glands of groin, neck and elbow.
3. Skin rashes.
4. Mucous patches in mouth and throat.
5. Headache and alopecia.

In practically all the new cases of syphilis a definite Hunterian chancre, an elevated indurated ulcerated sore, was easily demonstrated, and great clinical attention and importance was paid to the hard india-rubber induration on grasping the lesion between the forefinger and thumb. No ordinary ulcer or any soft sore uncomplicated with syphilis gives this indiarubber feeling on palpation.

The presence of headaches was always enquired into very carefully, and brought out the information that 14·7 per cent. of all cases had definite headaches. These headaches were mostly nocturnal, although a few men complained of marked diurnal headaches. They were mostly of an ill-defined character and not located to any one part of the head. The men described them as "a general pain all over the head." A few cases were definitely located as "vertical" or "frontal." All these men stated that after their first injection of "606" the headaches cleared off, and they had no further trouble with them.

The diagnosis in most of the cases was obvious from an ordinary clinical examination, and the aids of the microscope and serum test were

quite unnecessary, and for quite a while the serum test was unobtainable, owing to war conditions, for such a number of cases. In some cases we demonstrated the presence of the *Treponema pallidum* in the sores by means of dark ground illumination, and also had blood tests done at Rochester Row Military Hospital.

As regards the clinical examination and diagnosis, the following points were most carefully enquired into:—

1. The interval which had elapsed between the exposure to infection and the first appearance of the sore. This, of course, was complicated when several exposures at varying times were admitted by the patient.

2. The persistence of a sore or ulcer on the penis in spite of the usual treatment by cleanliness and ordinary antiseptics. When a sore not definitely syphilitic did not heal up in a few days it was regarded with suspicion and more extensive enquiries made.

3. The hardness of the primary lesion. The dense india-rubber or cartilaginous feeling when the sore is grasped. It has aptly been represented as a button or piece of india-rubber let into the tissue. In primary lesions underneath the foreskin, the peculiar manner in which the foreskin rolls back with a sort of elevated collar is typical.

4. The enlargement and shottiness of the glands in the groin on both sides, and the early involvement of the glands in the neck and the epitrochlear glands in both arms. These discreet shotty glands are not tender, do not tend to suppurate, and are not tied down by adhesions unless some septic complication has set in.

5. Secondary rashes on the skin, lesions in the mucous membrane of the mouth and throat, and condylomata, alopecia, etc.

In no case did we get any iritis or eye complications, either at admission or later on.

In most of the old cases, even when the disease had been contracted from 8 to 20 years before, enlargement and shottiness of the glands could be demonstrated. The scars of the original sores on the penis were quite visible and were valuable guides to diagnosis.

One valuable point in the diagnosis of any condition which can be regarded as secondary syphilis is that we have never found any one clinical condition, such as skin infiltration, or affection of mucous membrane or bones, without accompanying enlargement of the glands of the neck, groin, and elbow. These are invariably carefully felt for, in addition to other enquiries as to history of original disease, looking for scars, etc.

In the Connaught Hospital, the first injection of Hg. is always given just before the "606" injection. This ensures that the patient need not be disturbed should he be suffering from a marked reaction after the "606."

Prior to the injection of "606" no special preparation of the patient is considered necessary, but no patient is allowed to have a heavy meal for several hours before.

We do not find that a small amount of albumen in the urine, as shewn by the cold nitric test, is a bar to the injection of "606" or kharsivan. When the amount is at all large, the "606" is withheld for a week or two, and the patient is treated with mercury. A further examination of the urine then usually reveals a great improvement in the amount of albumen and the "606" is given.

A number of men whose urine did not clear up satisfactorily, and whose appearance did not indicate any advanced renal disease, were given the full dose of "606," even although the urine was loaded with albumen. The urine invariably cleared up after this treatment, indicating that the albumen was of syphilitic origin.

COURSE OF TREATMENT.

One complete course of treatment was given to each case where they were not removed from the Command. After one such course a case was considered "fit for Active Service," as he had no active signs and was unlikely to need more treatment for several months or a year.

During the period of treatment only 2·3 per cent. of all the cases treated needed to be readmitted for hospital treatment, the necessity for such being due to patches occurring in the mouth, probably due to excessive smoking, sore gums from too much Hg., or neglecting to keep teeth clean, or, as happened in a few cases, the original primary lesion broke down once more after having completely healed up.

TECHNIQUE OF PREPARATION OF THE SOLUTIONS OF THE ARSENICAL PREPARATIONS.

1. For neo-salvarsan or neo-kharsivan. All that is necessary is to boil some ordinary tap water, then take 10 c.c. of this at 60° F. and shake in the contents of the neo-salvarsan or neo-kharsivan bulb. It readily dissolves in the water.

2. For salvarsan or kharsivan. The usual procedure is to dissolve them in sterile distilled water saline made by dissolving 8·5 grammes of pure sodium chloride in one litre of sterile freshly distilled water. We take 100 c.c. of this saline solution and dissolve the contents of the "606" bulb in it by shaking it up in a bottle with 50 small glass beads.

This dissolved solution is then neutralized with 4 per cent. sodium hydrate solution, and we find that from 4·2 c.c. to 4·8 c.c. of the sod. hyd. solution is needed to just cause the precipitate to be redissolved. Then the bulk is made up with sterile saline solution to 250 c.c.

Owing to the lack of distilled water, due to the distilling apparatus being out of order and from the fact that boiled tap water could be used for neo-salvarsan, I finally came to the conclusion that saline prepared from boiled tap water might be used for the preparation of the kharsivan solution. Quantities of plain tap water are boiled and put in a large sterile glass jar, where a certain amount of lime is deposited when the water cools. The clear water is syphoned off, made into saline, and again boiled; it is then filtered through sterile gauze to get rid of the further lime salts deposited. It is now ready for use as a solvent for the kharsivan.

In practice abroad in the tropics, when I could not get distilled water I have used clean rain water off a new galvanized iron roof, boiled, made into saline, and used as a solvent.

No untoward results have occurred from the use of rain water or boiled tap water. To many men in out-of-the-way parts of the tropics this hint may prove of much use.

TECHNIQUE OF INJECTIONS.

When the patient is ready for the "606" injection, he is laid on the operating-table and the bend of the elbow is painted with a solution of iodine, 15 per cent. solid, in chloroform. Dissolving the iodine in

chloroform is recommended as the latter drug quickly penetrates the fatty material in the skin. A tourniquet consisting of a piece of rubber tubing is put round the upper arm, midway between the shoulder and the elbow; one turn only is required, and the ends are left (*vide Fig. 1*) so that it can be pulled off in one motion. He is then instructed to open and close his fist slowly, so as to cause the superficial veins to fill up.

Most of our patients have large outstanding veins which present no difficulty in puncturing, but occasionally one meets, especially in stout men, flabby fat arms with small veins which will not swell up by opening and closing the fist.

Several plans have been devised to distend these veins. One may direct the patient to stand up and swing the arm round the head several times, then stoop down while the tourniquet is being applied. Or, again, one may let the arm be held under the hot-water tap for several minutes prior to putting on the tourniquet. When these measures fail one has to put on the tubing tourniquet high up near the shoulder, and put on a few turns of an Esmarch bandage from the shoulder down and from the fingers upwards; this always pushes enough blood into the elbow veins to distend them. So far, in our experience, we have never had to cut down to expose the vein for puncturing.

For sterilization, all the apparatus has been boiled and the needles kept in 90 per cent. alcohol.

When the prominent veins at the elbow are distended, the operator picks up the skin over the largest vein and rapidly introduces the needle under the skin alone. The skin is then let down on the vein, and in turn the vein is entered by elevating the base of the needle, pressing the point against the top of the vein, and the needle slips in. *(Fig. 1.)*

As soon as the operator feels that the vein is punctured, he loosens the tourniquet by giving the long end one pull, and immediately turns on the saline, or, when giving small injections, presses the piston of the syringe and forces the solution into the vein.

Should the needle, by any mishap, have left the vein or gone through the wall on the other side, one sees the skin begin to elevate from the pressure of fluid underneath. If this occurs, the needle must be at once withdrawn and a fresh puncture made.

When the quantity of the solution is from 50 c.c. to 250 c.c, it is

always run in by gravity from the apparatus shewn. *(Fig. 2.)* When the medium is smaller than 50 c.c., it is put direct into the vein by means of a "Record Syringe." In this latter case it is advisable to put the needle into the vein by itself, make sure that it is in the vein by seeing blood run out of the base of the needle, and immediately fit on the syringe and inject.

When the larger solutions are to be run in by gravity, a small quantity of saline solution is first allowed to run into the vein: should this run freely the saline is turned off, the "606" solution turned on, and then, when the latter has run down to the Y-shaped tube, the saline is again turned on to wash any solution remaining in the lower tube into the vein, then when the lower "window" in the injecting tube shews clear saline solution only, the clip is shut. The needle is withdrawn now, a pad of sterile lint is placed on the puncture, the arm is raised up vertically to stop any bleeding from the vein and a collodion dressing is applied. *(Fig. 3.)*

After holding the arm up above the head for a few minutes the patient proceeds to bed.

These injections are usually done in the forenoon and the reaction and temperature noted during the rest of that day and the following. The temperatures are taken at 9 a.m., 3 p.m., and 9 p.m. Men attending from outside for "606" injections remain in hospital for four hours after. If then they shew no marked reaction they are allowed to rejoin their unit.

The total number of injections of salvarsan, neo-salvarsan, kharsivan, and neo-kharsivan given during the period were 2,095.

For the mercurial injections, all the syringes (all glass) and platino-iridium needles are sterilized in hot olive oil at 180° C.

A small nickel capsule full of olive oil is kept over a spirit-lamp or a bunsen flame. If necessary a thermometer is kept in this, but for all practical purposes, if the oil is heated until it just smokes, that means that its temperature is roughly about 180° C.

This way of sterilizing the needles and syringe is the most satisfactory, for the oil at this temperature will kill all living organisms, it also mixes perfectly with the grey oil, and, in addition, when sucked up in the syringe after the injection is over, it cleanses the needle from all particles of mercurial cream. In doing a series of cases it is not

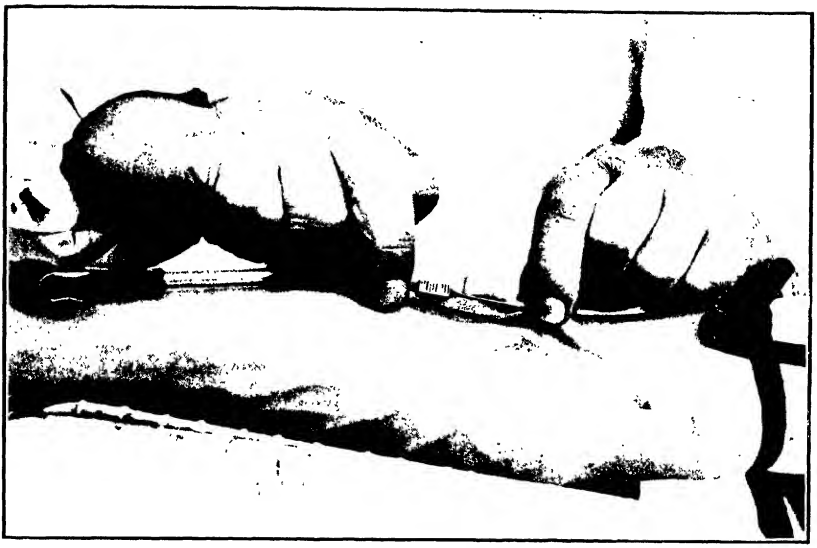


Fig. 1.

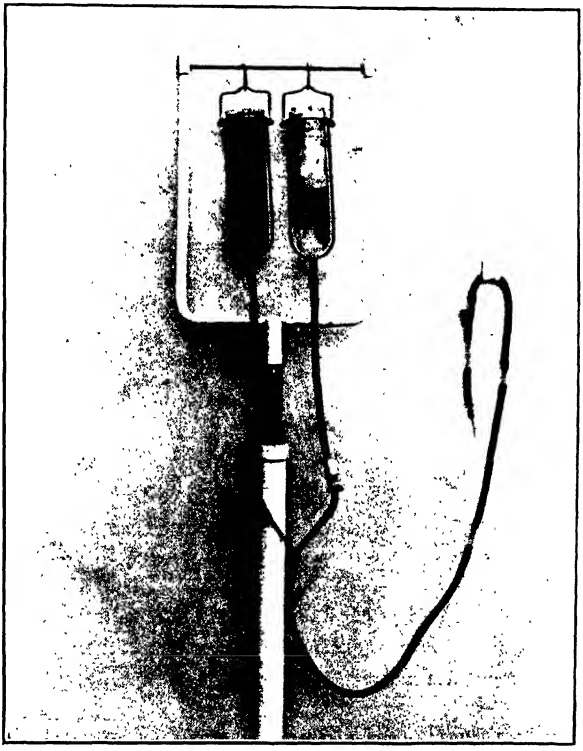


Fig. 2.

Illustrations to accompany Capt. H. J. McGrigor's Paper.

Fig. 3.

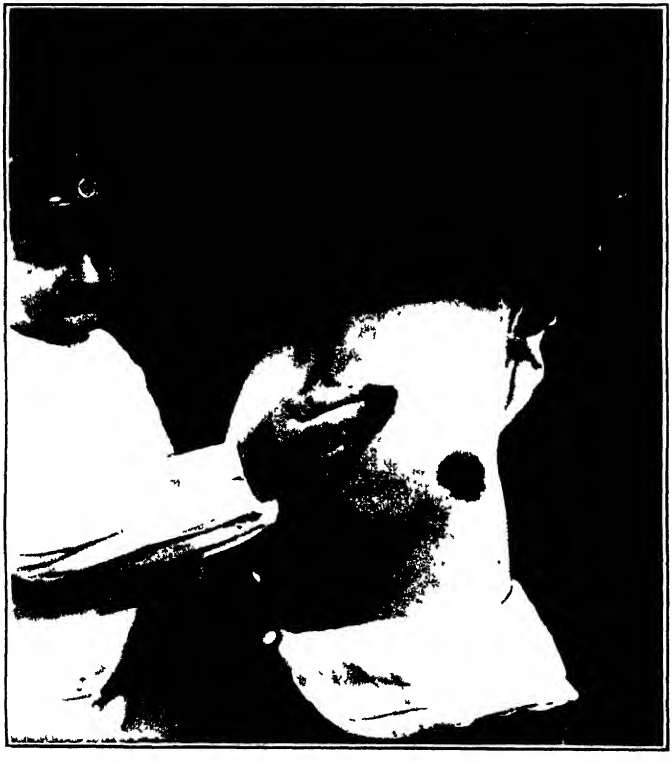


Fig. 4.

Illustrations to accompany Capt. H. J. McGregor's Paper.

necessary to sterilize the whole syringe for each case. It is sufficient to dip the needle into the hot oil between each injection to ensure that no infection in the needle remains alive.

On very few occasions indeed have we had any abscess formation occur, and only seldom do we have any painful thickening at the seat of injection. At this point I must impress the fact that if the injection is not made deeply into the muscle, there is a great tendency towards the formation of painful thickenings, especially if the injection be made into the fat and subcutaneous tissue only.

The materials being ready, a point on the buttock, about midway between the post superior spine of the ilium and the top of the great trochanter of the femur, is painted over with the solution of iodine, 15 per cent., in chloroform *(Fig. 4.)*. Then with a quick vertical plunge the needle is inserted at right angles to the surface and down into the muscle. The injection of 1 gr. of metallic mercury for an average-sized man once a week is found to be quite sufficient mercurial treatment.

During the period under discussion, 5,168 mercurial injections have been given.

All patients are warned as to the necessity of keeping their teeth clean, and to report any soreness of the gums at once. Where men have allowed their teeth to get into a dirty state with tartar and debris between them, they are set to work with pointed matches, cotton wool and finely-powdered pumice-stone to get them reasonably clean.

It has been the experience in this hospital that men who keep their teeth clean seldom suffer from soreness of the gums during their treatment.

Occasionally one comes across patients who have, or profess to have, conscientious objections to injection of any kind, or object to the same through ignorance or fear. In these cases, as one cannot compel them to submit to an injection, which is considered to be an operation, they are put on a course of inunction. In this method they have a daily rubbing of 30 grains of ung. hydrag. continued for a period of six weeks. The irksomeness of this method of treatment usually overcomes the objection to injection which was originally entertained.

Owing to the well-known fact that the injections of the arsenical compounds alone do not ensure a cure of syphilis, various combinations of arsenical and mercurial injections have been adopted. For a time

we gave 1 "606," then 9 Hg., followed by 1 "606." Then we tried 1 "606," 2 Hg., 1 "606," 2 Hg., 1 "606," and 1 Hg.

This latter method has the advantage of being completed in one month, while the former method takes nine weeks. I understand that Major FRENCH, O.C., Military Hospital, Rochester Row, favours the nine weekly injections of Hg. as shewing the better results.

LOCAL TREATMENT OF SYPHILITIC LESIONS.

The primary sore is kept clean by frequent washing with mercuric chloride solution (1/1,000). If foul to start with, it is cleaned by the application of petrol or ether; then a small piece of sterile lint wrung out of mercuric lotion (1/1,000) is applied.

In most cases this local treatment is sufficient. Cases which shew any delay in the healing process are treated by means of cataphoresis. Here a current of 10 milli-ampères for ten minutes is used, the positive pole being applied to the sore which is covered with a small piece of plain lint kept moistened with mercuric lotion (1/1,000). This speedily facilitates healing.

In many cases when the sore is situated on the foreskin a circumcision is done and the whole sore removed. So far as our experience goes, it shews that the removal of the sore itself does not in any appreciable way effect the course of the disease.

Occasionally some very extensive sloughing has been met with; in some, most of the glans had been eaten away. The thorough application of the actual cautery, or pure nitric acid, invariably changed the condition for the better.

The most striking fact in the healing of local lesions is the general treatment by the arsenical compounds. On several occasions when the supply of "606" became exhausted, the cases did not receive any such treatment for weeks. At once one was struck, in going round the wards, to see the delaying of the healing of the sores. Again, when the new stock had been received, the rapid improvement after the injections were recommenced was equally, if not more strikingly, noticeable.

The ordinary syphilitic ulcerations of the skin are treated on the usual lines—antiseptics, mercurial ointments and drying powders.

Condylomata when extensive are cauterized and mercurial ointment applied. When small they are kept powdered with calomel.

Mucous patches on the lips, tongue, buccal mucous membrane, and tonsils are touched with silver nitrate (ʒ1/ʒ1) solution. Mouth washes of alum, potassium chlorate and potassium permanganate are also employed.

These applications to the local lesions all have their sphere of usefulness, but are of little or no avail unless the general treatment of mercury and "606" is actively pursued at the same time.

It has been noted that tertiary lesions, especially those deep ulcerations on the face near the nostrils and lips, heal with really remarkable rapidity when the "606" injections are given. Even in twenty-four hours a very marked improvement could easily be seen.

REACTIONS AFTER INJECTION OF "606," NEO-SALVARSAN, AND KHARSIVAN PREPARATIONS.

After injections of the arsenical preparations the reactions noted were: rigor, headache, vomiting, diarrhœa, and the presence of raised temperature on day of injection and following day.

These reactions are summarised in tables attached.

An examination of the results in these tables, which represent large numbers of injections, is shewn in the different series according to the nature of the preparation, the quantity of the medium, and the quality of the medium.

A very cursory look at them at once strikes one that the results, as expressed in reactions, are most varied, and that it is most unprofitable and would be a waste of time to compare them reaction by reaction.

In order to make an attempt to bring them to some sort of order, I have devised the plan to consider them in the following way—these reactions are all expressed as percentages, and most of them are compiled for series of 100 cases, some in greater numbers, none in less, except in 3a, 10 and 13.

By adding the sum total of the figures representing the rigors, headaches, etc., and dividing that total by the number of cases in each table which had a reaction, one gets a figure which may be said to express the average number of reactions in each series.

For example. In contrasting the results from the injection of kharsivan in 250 c.c. distilled water saline with those from the same

preparation in 250 c.c. boiled tap water saline, one notes that the result is in favour of the latter in the proportion of:—

	1st injections	1·66	against	2·28
Against in	2nd ,,	2·50	,,	1·50
In favour	Total ,,	1·71	,,	1·80

(*Vide* Tables—Series 5 and 14).

the discrepancy in the second injection results being due to the fact that of these second injection cases only two had reactions and they were both multiple reactions. In no case did reports come in from outside units of any men reporting sick after such second injection.

Another example. When kharsivan was dissolved in 100 c.c. distilled water saline as against 100 c.c. boiled tap water saline, the result shewed in favour of B.T.W.S. in the proportion of:—

1st	injections	1·71	against	2·33 D.W.S.
2nd	,,	1·37	,,	1·90 ,,
Total	,,	1·56	,,	2·27 ,,

(*Vide* Tables—Series 6 and 12).

Another example. When kharsivan was dissolved in 50 c.c. D.W.S. as against 50 c.c. B.T.W.S., the result shewed in the proportion of:—

1st	injections	1·43 D.W.S.	against	1·69 B.T.W.S.
2nd	,,	1·51 B.T.W.S.	,,	2·18 D.W.S.
Total	,,	1·68 ,,	,,	1·93 ,,

(*Vide* Tables—Series 8 and 11).

Still another example. In contrasting the results from injections, when kharsivan was dissolved in 20, 50, 100 and 250 c.c. D.W.S., they shew in favour of 20 c.c. in the proportion of:—

1st	injections	1·71,	against	1·90	50 c.c.,	2·33	100 c.c.,	2 28 250 c.c.
2nd	,,	0·95,	,,	2·18	,,	1·90	,,	1·50 ,,
Total	,,	1·70,	,,	1·93	,,	2·27	,,	1·80 ,,

(*Vide* Tables—Series 5, 6, 7, 8 and 9).

Again, in comparing these reactions from the injections of salvarsan and kharsivan in 250 c.c., we get these figures in favour of salvarsan in distilled water saline:—

1st	injections	1·61,	against	2·28	K. in d.w.s.	and	1·66	K. in b.t.w.s.
2nd	,,	1·27,	,,	1·50	,,	,,	2·50	,,
Total	,,	1·39,	,,	1·80	,,	,,	1·71	,,

(*Vide* Tables—Series 1, 2, 5 and 14).

It was impossible to carry out the experiments in detail with the salvarsan in the earlier part of the war, so we cannot compare results in lower dilutions of salvarsan with kharsivan.

In the tables one ought to note the number of cases where *no reaction* at all occurred in each class of injection This is very important. By this is meant no rigor, headache, vomiting diarrhœa, and no temperature over 100° F.

The total knowledge gained by the series of experiments is not very great, but what is gained is the fact that the reactions are really not much affected by the quantity or quality of the medium used, and that the experience here has been that one can use boiled tap water with safety and can reduce the quantity of the medium used with safety and even profit.

Perhaps these results will be of use to many members of the Society who are stationed abroad in the tropics where one can easily obtain the drugs by post, but where the provision of the distilled media may be impossible.

I know by personal experience the difficulty, in out-of-the-way tropical places, of obtaining distilled water, and hope that my results may point out a way of overcoming this.

The introduction of various remedial agents by intravenous injection is not by any means limited to the treatment of syphilis—for example, trypanosomiasis and kala-azar treated by antimony—and the practical results of my series of experiments may perhaps lead other men to follow up tropical diseases treatment by an extension in the use of such injections.

TABLE I.—Percentage of First Injections.

Series No.	Preparation and Medium.	Rigor.	Headache.*	Vomiting.*	Diarrhœa.	Temp. over 100° F. 1st Day.	Temp. over 100° F. 2nd Day.	No Reaction.	Severe and Multiple Reaction.	Highest Temp. Recorded.	Average Reaction Figure.
1	·6 Salvarsan, 250 c.c., D.W.S. ..	10·34	43·10	5·17	4·31	56·03	12 93	28 20	8·62	104·2 R H	1·83
2	·6 ,, 250 ,, ,,	14·37	1·72 29·34	11·67	3·29	54·49	8·08	25·44	5·68	104 0 V D	1·61
3	·9 Neo-Salvarsan, 10 c.c., D.W.S. .	2·80	23·30	2·80	3·70	18·70	4·60	62·62	2·70	102·0 H V	1·51
†3A	·9 ,, 10 ,, ,, .	0	0	0	0	0	0	100 00	—	—	0
4	·9 Neo-Kharsivan, 10 c.c., D.W.S.. .	11·76	28·23	9·41	8·23	34·11	11·76	41·17	10·58	104·0 V	1·75
5	·6 Kharsivan, 250 c.c., D.W.S. . .	25 92	3·52 38·27	30·86	19·75	49·38	6·17	20·98	28·39	104·6 R H V D	2·28
6	·6 ,, 100 ,, ,,	2·85	11·11 28·57	34·28	14 28	54·28	17·14	34·28	37·14	104·2 H	2·33
7	·6 ,, 20 ,, ,, ..	1·26	3·71 21·51	2·85 3·79	5·06	12·92	5·06	70·88	15·18	103·8 V D	1·71
8	·6 ,, 50 ,, ,,	0	6·32 7·02	10·81	10·81	32·43	16·21	45·94	16·21	102·8 H V	1·43
9	·6 ,, 50 ,, ,,	10·90	5·40 30·90	10·90	0	34·54	7·27	50·90	18·18	104·8 R H	1·90
‡10	·6 ,, 50 ,, ,, . .	0	9·09 0	0	0	0	0	100·0	—	—	0
11	·6 ,, 50 ,, B.T.W. .	10·25	35·89	2·56	7·69	41·02	7·69	38·46	12·82	104 0 H V	1·69
12	·6 ,, 100 ,, ,, ...	2·63	7·69 26·31	15·78	18·42	28·94	15·78	36·82	13·15	104·0 H V (2 Cases) R H	1·71
13	·6 ,, 100 ,, D.W.S. ..	0	11·12	0	0	0	11·12	86·66	—	102·2 H	1·71
14	·6 ,, 250 ,, B.T.W. ..	0	46·00	12·00	2·00	32·00	8·00	40·00	6·00	102·4 H V (2 Cases) H D	1·66
15	·6 ,, 100 ,, D.W.S. ..	0	36·00	13·33	0	30·00	10·00	50·00	16·66	102·6 H V	1·78
16	·6 ,, 100 ,, ,, . .	2·70	3·00 48·64	18·91	0	32·43	5·40	43·24	18·91	102·4 R (2 Cases) H V	1·89
17	·6 ,, 100 ,, ,, .	0	23·80 9·52	4·76	0	23·80	4·76	71·42	9·42	104·0 (Sev. H)	1·96

* A second figure in these columns means that that percentage had severe headache and vomiting.
† Only 17 injections in this series. ‡ Only 6 injections in this series. R = Rigor. H = Headache. V = Vomiting. D = Diarrhœa. This Table refers to 100 injections or more, except Nos. 3A, 10 and 13.
D.W.S. = Distilled Water Saline. B.T.W. = Boiled Tap Water Saline.

TABLE II.—Percentage of Second Injections.

Series No.	Preparation and Medium.	Rigor.	Headache. *	Vomiting.	Diarrhœa.	Temp. over 100° F. 1st Day.	Temp. over 100° F. 2nd Day.	No Reaction.	Severe and Multiple Reaction.	Highest Temp. Recorded.	Average Reaction Figure.
1	·6 Salvarsan, 250 c.c., D.W.S. ...	0	0	0	0	0	0	100·00	0	0	0
2	·6 ,, 250 ,, ,, .	1·44	8·17	6·73	0·48	27·88	0	64·90	0·48	103 0 Nil.	1·27
3	·9 Neo-Salvarson, 10 c.c., D.W.S.	0	0	2·30	0	0	2·30	95·34	—	104·0 Nil.	0·92
3A	·9 ,, 10 ,, .,	0	0	0	0	0	0	100·00	—	—	0
4	·9 Neo-Kharsivan, 10 c.c., D.W.S.	0·91	18·34 7·33	0·91	11·00	3·66	2·75	71·55	11·92	101·4 H +	1·29
5	6 Kharsivan, 250 c.c,, D.W.S.	2·18	19 67 3·82	13·66	10·38	19·12	1·09	55·73	10·38	103 6 V 2 Cases) V	1·50
6	·6 ,, 100 ,, .,	0	4·61	4·61	0	6·15	4 61	90·76	4·61	103 0 V	1·9
7	·6 ,, 20 ,, ,.	0	4·76	0	0	0	0	95 23	—	100·0 H	0·95
8	·6 ,, 50 .. ,. ..	0	12·69	7·93	1 58	6 34	6 34	84·12	9·32	101·0 H	2·18
9	·6 ,, 50 ,, ,,	0	2 22	0	0	0	2·22	97 77	—	—	2·22
10	·6 ., 50 ,, .,	0	0	0	0	0	0	100 00	—	-	0
11	·6 ., 50 ,, B.T.W.	1·63	6·55	6·55	0	1·63	3·27	86·88	3·27	101 0 —	1·51
12	6 ., 100 ,, ..	0	17·74	3·22	3·22	8 06	4·83	72 58	—	101 0 D	1·37
13	6 ., 100 ,, D.W.S	0	0	0	0	0	0	100·00	0	—	0
14	·6 ,. 250 ., B.T.W.	2 00	4·00	2·00	0	2 00	0	96 00	4·00	101 4 R H	2·50
15	·6 ,. 100 ,, D.W.S.	1·42	2·87	1·42	1·42	4·28	0	92·85	—	100·4 D	1·63
16	·6 ,, 100 ,, ., ..	0	3·17 1·58	1·58	0	0	1·58	95·23	3·17	100 4 —	1·26
17	·6 ,, 100 ,, .,	0	0	0	0	0	0	100·00	—	100·0 —	0

* A second figure in the headache column means that that percentage had severe headache.
R = Rigor. H = Headache. V = Vomiting. D = Diarrhœa.
This Table refers to 100 injections or more, except Nos. 3A, 10 and 13.

TABLE III.—Percentage of Total Injections.

Series No.	Preparation and Medium.	Rigor.	Headache.*	Vomiting.*	Diarrhœa.	Temp. over 100° F. 1st Day.	Temp. over 100° F. 2nd Day.	No Reaction.	Severe and Multiple Reaction.	Highest Temp. Recorded.	Average Reaction Figure.
1	·6 Salvarsan, 250 c.c., D.W,S.	10·15	42·73 1·70	5·12	4·27	5·55	12·82	28·20	8·54	104·2 R H	1·12
2	·6 ,, 250 ,, ,.	9·40	21·21	9·77	2·21	46·30	4·98	40·03	3·69	104·0 V D	1·39
3	·9 Neo-Salvarsan, 10 c.c., D.W.S.	2·00	16·60	2·60	2·60	13·30	4·00	72·00	2·00	102·0 H V	1·47
3A	·9 ,, 10 ,, ,,	0	0	0	0	0	0	100·00	—	—	0
4	·9 Neo-Kharsivan, 10 c.c., D.W S.	5·67	22·68 5·67	4·63	9·79	17·01	6·70	58·24	11·34	104·0 V	1·57
5	·6 Kharsivan, 250 c.c., D.W.S.	9·46	25·37 6·06	18·93	13·25	29·46	2·65	45 07	15·90	104·6 R H V D	1·80
6	·6 ,, 100 ,, ,.	1·00	13·00 2·00	15·00 1·00	5·00	23·00	9 00	71·00	16·00	104 2 H	2·27
7	·6 ,, 20 ,, ,,	1·00	18·00 5·00	3·00	4·00	11·00	4 00	76·00	12·00	103·8 V D	1·70
8	·6 ,. 50 ,, ,,	0	18·00 2·00	9·00	5·00	16 00	10 00	70 00	12·00	102·8 H D	1 93
9	·6 ,, 50 ,, ..	6·00	18·00 5·00	6·00	0	19·00	5·00	72·00	10 00	104·8 R H	1·42
10	·6 ,, 50 ,, ,,	0	0	0	0	0	0	100·00	—	—	—
11	·6 ,, 50 ,, B.T.W.	5·00	18·00 3·00	5·00	3 00	17·00	5·00	68·00	7·00	104·0 H V	1·68
12	·6 ,, 100 ,, ,, ...	1·00	21·00	8·00	9·00	16·00	9·00	59·00	5·00	104 0 H V (2 Cases) R H	1·56
13	·6 ,, 100 ,, D.W.S.	0	6·66	0	0	0	6 66	93·33	—	102·2 H	1·90
14	·6 ,, 250 ,, B.T.W.	1·00	25 00	7 00	1 00	17·00	4·00	68·00	5 00	102·4 H V (2 Cases) H D	1·71
15	·6 ,, 100 ,, D.W.S.	1·00	13·00 1·00	5·00	1·00	12·00	3·00	80·00	5·00	102·6 H V	1·75
16	·6 ,, 100 ,, ,,	1·00	20 00 1·00	8·00	0	12·00	3 00	76·00	9 00	102·4 R (2 Cases) H V	1·83
17	·6 ,, 100 ,, ,, ...	0	10·00 4·00	2 00	0	10·00	2·00	88·00	4 00	104·0 H +	2·33

* A second figure in these columns means that that percentage had severe headache or vomiting.
R = Rigor. H = Headache. V = Vomiting. D = Diarrhœa.
This Table refers to 100 injections or more, except Nos. 3A, 10 and 13.

Dr. G. PERNET: As none of the military men present are coming forward, I should like to say a few words as a civilian. I have been very greatly interested in Capt. MCGRIGOR'S paper, and I should like to congratulate him upon it. For many years I have used grey oil, and I have written on the point many times.*

There is one point I should like to emphasize, and that is, the great advantages military surgeons have over civilian practitioners. They deal with patients who are in the prime of life, selected individuals; and, in the second place, they have them under complete control. In hospital practice, we have no such control; patients come one day and follow up the treatment for a little while, and then they do not turn up again. It would be impossible to get excellent tables, like those presented, worked out in the ordinary general hospitals in London.

One point about salvarsan is that it whitewashes patients almost too well. In civil hospitals, they get rid of the ulceration and rashes rapidly by such means. Unfortunately they do not always turn up again, and you cannot follow up the treatment. The result is that in many of these cases, from the very fact that the treatment has improved them, really you are indirectly doing them an injury by letting them think that all is right, so that they do not follow up the treatment. The other day a woman turned up: she had broken down gummata about the left knee and on the right thigh. I found she had been in hospital four years previously, had had one injection of "606," and had never had any other treatment for her syphilis, but had been running about loose ever since. That is where the military surgeons have a great advantage over us. They collect their figures through men who are also under discipline, and the observations are regularly maintained day by day. For that reason, such tables as those which Capt. MCGRIGOR has given us are of great importance as guides in the treatment of syphilis.

I noticed there were few cases of ordinary chancroids—124 compared with much larger figures for syphilis and so forth. In the London hospitals, for instance, it is extremely rare to see chancroids. Directly you get people together (international exhibitions) then chancroids increase. In every case of chancroid, one must keep a sharp look-out

*PERNET: The treatment of syphilis, with special reference to intra-muscular injections, *British Medical Journal*, vol. I. 1907. The intra-muscular treatment of syphilis, *Lancet*, vol. II. 1909.

for the possibility of mixed infection, and syphilis developing later on.

Another point that struck me about Capt. McGRIGOR'S cases is the absence of iritis. Syphilitic iritis is not very common as far as my experience goes—I only see it occasionally. It is instructive to find from the figures given in the paper that no single case of iritis or other ophthalmic complication has occurred.

The cases with the albumen in the urine are instructive, because it shews that syphilis is not infrequently complicated by specific albuminuria. There is a difficulty in civilian practice with patients who have had nephritis before contracting syphilis. Then you have to keep your hands off salvarsan.

Capt. C. H. MILLS: I would like to say a few words in the discussion upon Capt. McGRIGOR'S very interesting and detailed report of his work at the Connaught Hospital, Aldershot. For the past sixteen months I have been in charge of the Syphilis Wards at Rochester Row Military Hospital, and am glad of this opportunity of being able to compare the results obtained in the employment of the new organic arsenic preparations at the two hospitals.

One of the first points that struck me in this report was the extreme ease with which the diagnosis was arrived at in the great majority of cases. This was not my experience. So many have been the cases at Rochester Row in which the primary sore has been anything but typical, that I have made it a routine to arrive at a positive diagnosis only by detecting the *S. pallida* by the dark-ground illumination. I would say, without looking at statistics, that the proportion of chancroids to the syphilis chancre has been even higher in my cases, and that masked mixed infections are also very frequent. Only by the use of the dark-ground illumination as a routine can time be saved in cases which would otherwise have to be kept under observation—free from local antiseptics possibly for weeks. It is a simple matter to decide that the chancre is definitely syphilitic when typical, but it is extremely difficult—usually impossible—to decide that a sore is definitely non-syphilitic, when relying purely upon its clinical characteristics. This is especially the case when so many have had local antiseptics applied; and so I urge the importance of the dark-ground examination—whether from the chancre, gland puncture, or blistered suspicious rash—otherwise one

must perforce sit down and watch for possible syphilitic manifestations for perhaps weeks. The WASSERMANN tests in early suspected syphilis, if negative, are valueless.

When the chancre is typically indurated, or when the secondary manifestations are evidenced in abundance, further investigation is needless. In tertiary cases we always check the diagnosis with the WASSERMANN test. Another point in Capt. MCGRIGOR'S paper is that no appreciable difference in the reaction is produced when using boiled water instead of distilled for the medium in the arsenical preparations. Were this to be employed indiscriminately throughout the country, I am sure that in many instances serious reactions would occur. It had to be abandoned at Bulford, there being wide variations in the impurities of different water supplied, the salts in some possibly altering the chemical composition of the drug employed. Again, on hearing the very slight reaction produced in these cases, the civilian practitioner could very easily form an erroneous impression.

It is a recognised fact that soldiers tolerate—say, an injection of a drug like kharsivan—very much better than would the average type of patient one meets with as out-patients. To administer an injection of kharsivan to an emaciated pauper, and only retain him in bed for four hours would be asking for trouble.

I did not hear much mention in the paper of tertiary cases nor of cerebral, or tabetic cases; we get a fair proportion of these. My experience has been, especially in the latter types, that one should never put them right away on salvarsan. We have all met with, or heard of, cases of cerebral-syphilis in which there has been an alarming increase of symptoms, even culminating in death, following upon intravenous injections of the organic arsenics. My explanation is that a HERXHEIMER'S reaction is produced in an artery or arteries, already almost occluded by syphilitic endarteritis, thereby bringing about its total occlusion, and depriving an area of already impoverished brain of any blood supply at all. I always put a cerebral-syphilitic or tabetic on mercurial inunctions and pot. iodid. for at least fourteen days, to promote an absorption of the small round cell infiltration in the arterial walls before commencing "606" treatment.

We are at present working upon a series of tabetic cases by giving intrathecal injections of mercurialised serum combined with the usual

systemic treatment, and further trying salvarsanised-mercurialised-serum. This we prepare by drawing off sufficient blood to give 30 c.c. of serum in a patient who has previously had "606" administered. To the serum we add 1/25 of a grain of hyd.-perchloride. The albuminate forms at first, but can be made to redissolve. This fluid, consisting of salvarsanised serum, plus antibodies, plus mercury, is injected intrathecally at weekly intervals, three injections being given.

The chief difficulty is to get cases in the early stages; most of them have gone too far to hope for anything more than to bring about an arrest of the degenerative processes. At present I have got seven cases that I have subjected to this treatment, and in every case I can say that I have been very pleased with the results, the symptoms being definitely relieved, even should this only prove to be temporary. Of course, one had to remember the withdrawal of cerebro-spinal fluid will often do this. One case of tabes with perforating ulcer has done very well. The rectal and bladder crises have cleared up, and the ulcer itself—always a pretty good test of a treatment—is now practically healed. Another case with a hypertrophic Charcot's hip, which was extremely painful—a rare symptom—is now quite painless, and the effusion greatly diminished.

Concerning Dr. PERNET'S enquiry with regard to the intra-muscular injections of mercury, I think this is the most satisfactory way of giving mercury to soldiers. One knows exactly *how much* the patient receives, and *when* he receives it. Years ago they were given pills to take, and, I am told, as a result that the drains of the hospital became blocked with pills, the men never even taking them off the premises. I do not think there are any other points I can think of now. Most of the figures seem to compare very well with my experience at Rochester Row. I did not notice whether Capt. MCGRIGOR had given many injections of neo-kharsivan. The worst reactions that we have had occurred with this drug, and we have now abandoned it. We have had two deaths following the employment of this drug, in each instance pneumonia set in at the finish. The symptoms developed as a profuse exfoliative dermatitis accompanied by intense jaundice. In one case I performed a post-mortem examination. The liver and spleen were engorged and swollen, and there was a marked hæmorrhagic pleurisy. Both cases were hypostatic. There were also petechial hæmorrhages in the meninges. The other case was transferred to another hospital.

We find galyl by far the safest of the new arsenical compounds, and very efficacious clinically. I have not yet seen herpes develop after its administration, which is an extremely common occurrence after kharsivan. I consider that many of the reactions noticed with these drugs only occur when there is a risk of the solution being too acid. None of them seem to develop with an alkaline solution which, of course, galyl gives.

Dr. T. P. BEDDOES: With regard to intravenous injection of "606" and alternatives, no doubt there is a certain amount of difference between treating robust soldiers and treating the average class of patient one comes across in private practice and non-syphilitic cases with injections other than "606." With regard to preparation, an anxious patient takes no food at all for some time before, and in that condition has a greater tendency to vomit. Patients injected in the morning should take just a small breakfast—a cup of weak tea and some dry toast. No heavy meal, but always a light one.

The next point is in the binding of the limb. Capt. MCGRIGOR uses a bandage tourniquet, which he ties with a half twist, and then pulls that off. If he has had one experience as I had: somebody else pulled off the bandage and jerked out the needle; no other vein could be found in the arms or hands, but fortunately one was found at the ankle. That is an experience one does not wish to repeat. In most cases it is better to search for a vein in every possible position before cutting down on one at the bend of the elbow. With pneumatic pads there is a certain amount of trouble. To distend the vein I use a broad rubber bandage, fastened with a safety pin, running the safety pin through the bandage but not fastening it. This gives a good hold, and can be pulled off with one finger. This, I think, is the best way; with the half hitch you are liable to get the needle jerked out.

Then Capt. MCGRIGOR says he is satisfied when he feels the needle is in the vein. It strikes me there is a certain amount of risk about that. You may be sure if you have a large vein—but, take a small vein. It is, I think, desirable to have the reservoir lowered below the level of the patient, and see the blood at the first window, the one next the needle. As to Y-shaped arrangements: all these Y-shaped arrangements are not as convenient as a single receptacle, and you cannot so easily watch

whether it is filled or not as when you have a narrow receptacle. Secondly, it means a complicated arrangement of taps. Now, if the receptacle is fixed by a hook on to various pegs—the handles of drawers do extremely well for this—you can raise and lower it much more easily than with a Y-shaped tube. The one I use is narrow below, with a funnel-shaped upper opening. You can tell more easily when the contents are falling, there is no risk of spilling it, and you can raise and lower it with one hand. Then you keep on filling it until your salvarsan solution is finished. Then it is filled with saline, and when the fluid at the first window is clear the needle is withdrawn.

Then the suggestion, when using concentrated solution, as to feeling the needle in the vein. There is a certain risk in that—it may have passed through the vein. But if the syringe, with the solution and needle attached is put into the vein, and the piston drawn back, if the needle is in the vein the blood is seen in the syringe and the solution is then at once injected; to put the needle into the vein and afterwards attach the syringe to it, is, I think, liable to cause a certain amount of movement which may drive the needle through the vein; and although it may be a minor point, and may not occur in hospital work as in private practice, it seems, after seeing and trying both methods, best to fix the needle to the syringe before puncturing.

Dr. R. A. O'BRIEN: I am afraid I have not very much to say on the matter. I had the duty of testing kharsivan when it was first made; it was rather a difficult matter, as very little experimental work had been done in England on these later organic arsenical preparations, and careful search of EHRLICH'S writings did not reveal in detail the criteria actually demanded by him. Of course, salvarsan theoretically is a chemical preparation and has a definite formula, and, therefore, every hundred samples of salvarsan should give technically the same analysis. As a matter of fact they do not. There is a considerable variation between individual samples. The arsenic content may vary considerably and that made the difficulty of testing much greater. One could not in the earlier stages rely on chemical analysis of these drugs. One had to test them on every laboratory animal available; we tested also with syphilitic rabbits. Mice infected with trypanosomes were used for testing every batch in the earlier days, but further details of testing will not interest you.

It is interesting to know that many thousands of injections of kharsivan have been given. One knows that some batches have undoubtedly produced more reaction than others. It was very difficult to get comparative statistics as to the reactions caused by different batches of the drug, and on enquiry as to how the distilled water was made we found that, although distilled water saline was used in most cases, the provision of satisfactory distilled water was in some instances definitely defective. Distilled water was perhaps made up by the dispenser, and used day after day. I know of two instances where the medical man who injects insisted on getting a glass still and redistilling the water himself. The second distillation takes place on the day of the injection. His results were remarkably good, reactions being rare. It seems to me from observation of such statistics and from the report on venereal diseases just published, that intense care should be taken to have satisfactory saline. As Capt. McGRIGOR points out, people on the outskirts of the Empire cannot get doubly distilled water saline, so they have to do the best they can. Where opportunity offers, it seems to me one is taking a very much less risk by using distilled water saline.

Mr. J. CANTLIE: I am sure you will agree with me in thanking Capt. McGRIGOR for a most instructive paper. We do not often get a paper of such completeness. We have had the absolute details given us—nothing is left to the imagination; so that a man who had never given a salvarsan injection before could understand how to do it readily. It is very interesting to find that an arsenical preparation (salvarsan) by itself is not so efficacious as when it is combined with mercury, and *vice versa*. The idea of one drug one disease is slowly but surely dying out; we have tried it for the last thirty years, but scientific research is stamping out a phase of medical practice of which we have good reason to be ashamed. We have tried to cure, of recent years, malaria with quinine alone; we tried to cure trypanosomiasis with antimony alone, and failed; We have recently added another to the list, viz., the cure of dysentery by a few injections of emetine, to the exclusion of the other ingredients of ipecacuanha, and it is not wholly satisfactory. Malaria was never treated by our predecessors with quinine alone; in the malarial districts of England the "fever powders" contained arsenic, opium and mercury in addition to quinine. The mercury was sometimes given beforehand as a

calomel pill; but mercury remains in the system for a few days before elimination, so, whether given before or with the powder, the effect was the same as a parasiticide when acting in combination with the other drugs. The question of the one drug one disease is being hit hard in the most scientific manner. Chronic malaria to-day is best treated by the combination mentioned above as the "fever powders." Warburg's tincture (without the quinine) has been in existence and widely used as the "fever mixture" since the days of Mithriades, some four thousand years ago, and it retains its position as a remedy. It is not of the one drug one disease category, for it contains over twenty ingredients—a tribute to the plurality of drugs in treatment, which has been supported by modern research on every hand.

Many points in Capt. McGRIGOR'S paper are excellent. The detail about raising the skin over the vein when the hypodermic needle is being inserted is a very wise precaution, for if the skin is at all tough, and the needle is pressed forcibly through, it is well to remember that beneath the median-basilic vein the brachial artery is in close juxtaposition; and as by the lancet, in days gone by, the artery was occasionally wounded when the vein was opened for venesection, so the hypodermic needle may find its way into the brachial artery unless Capt. McGRIGOR'S precaution is taken.

Dr. PERNET aptly referred to the differences as regards observation and control of patients in civil and military hospitals, to the great advantage of the latter. In civil hospitals, and in private practice, patients are under but little control as regards attendance, and subsequent histories are difficult to obtain. In military practice the reverse is the case, for as long as a man remains in the Army his medical sheet gives a continuous account of his illnesses and condition. We, therefore, have to look to our military confrères for much enlightenment in matters of this kind.

The one great objection to salvarsan treatment is that, with the rapid disappearance of signs and symptoms, the patient imagines he is cured by a few injections, and often refuses to take the treatment at a later stage, when the inevitable evidence of the disease returns. It is the same with the bath treatment at Aix and other places. The immediate signs and symptoms disappear, and when they return, as they invariably do if no further treatment is kept up, the patient refuses to believe that he is suffering from syphilitic recurrence, because he says he was completely

cured of that long ago by six weeks at Aix, etc. Many a man refuses anti-syphilitic treatment to the detriment of his health and that of his family, should he have children. Capt. McGRIGOR has assumed the *rôle* of teacher, and has shewn that he is proficient in that rare faculty. This paper should be in the hands of every practitioner, so that he can have no excuse in not undertaking the treatment advocated by Capt. McGRIGOR with so much precision and acumen.

Capt. H. J. McGRIGOR: In reply to Dr. PERNET, I quite appreciate the fact that our cases in the Army are selected, healthy young men, except a few broken down old men, and this makes a great deal of difference in the treatment of the disease, and even more so in the following up of the cases. We have our patients under control, and can get them at any time to attend for further treatment in case of relapse. With regard to his remarks about venereal sores, this paper deals with syphilis only, but I may state that during the period this paper covers I have not seen any cases of chancroid. Capt. MILLS referred to examining all the cases by means of the dark-ground illumination. As I have said in the paper, very few of our cases needed anything in the way of diagnosis beyond a careful clinical examination. Indefinite cases are examined by the microscope as a matter of routine. Regarding men shewing no reaction up to four hours after injection, I fear I did not make it quite clear in the paper that these men who come to hospital for the second and third injection, as a rule do not react after that period of time. It is well recognized that the reactions are much less in degree after the second or third than after the first injection. He also remarked on the difference of composition of tap water in different localities. To a large extent, any interference with the "606" would be by lime and carbon dioxide. The most of these are got rid of by the two boilings in the preparation of the saline.

Dr. BEDDOES did not quite seem to grasp the meaning of the single turn of the tourniquet on the arm. The way I described prevents any displacement of the needle, for it is steadied all the time by the right hand, while the left hand clears away the rubber band by a gentle pull on the long end. With respect to his plan of drawing in some blood into the syringe when giving neo-preparation, or into the end-piece in the gravity apparatus, there is always the possibility of the blood clotting

in and blocking up the needle, necessitating its withdrawal and re-entry. In reply to Dr. O'BRIEN, I may state that adverse reactions were not at all marked after the use of kharsivan or neo-kharsivan. After 1,285 kharsivan injections, only two cases reacted severely: one had an extensive dermatitis of both hands, which cleared up in 72 hours; and the other developed an arsenical neuritis of both legs, leading to paraplegia, from which he recovered in six weeks. Many cases did not react at all. This makes one doubt whether the reaction is much influenced by the medium in which the drug is administered. Labial herpes is not uncommon. The CHAIRMAN emphasized the importance of combining the use of arsenic and mercury in the treatment of syphilis, and also the treatment of malaria by means of arsenic and quinine, in this latter disease, especially in chronic cachectic conditions, a small dose of salvarsan can be usefully employed. Recently I have seen several patients treated with kharsivan alone, no mercury being given at all. Their condition, compared with others treated with kharsivan and mercury, was an object lesson in "how not to do it."

Illustrations to accompany Dr H Bayon's Paper

THE TREATMENT OF SYPHILIS IN AFRICAN NATIVE TRIBES AND AMONG NATIVE WORKERS ON THE RAND.

By Dr. H. BAYON.

Several attempts have been made to cope with the spread of syphilis among native tribes, at different times, by various Colonial Governments. The results have not always been commensurate with the effort and expense. The reasons are multiple and complex, but probably culminate in the fact that, when dealing with great numbers, and with an easily acquired disease, curative measures must be accompanied by prevention to achieve a lasting result. In addition, no system of treatment will be popular among African Natives* which is not only efficacious but also promptly evident in action. A painful treatment, even one that is dangerous, will not deter raw natives, if the medical man has their confidence, as long as evidence of cure is visible within a few days. Prolonged courses of treatment, however efficacious they may prove in the end, are extremely difficult to carry out. This we know from experience in the treatment of sleeping sickness with atoxyl or its substitutes.

The prevalence of syphilis among raw natives, living far from contact with white men, has often been exaggerated. Of course, differences exist, even between tribes living in contiguous districts, but, as a rule, one in twenty would be a fair proportion of clinically recognizable syphilitics among pagan natives. The proportion is certainly much higher among semi-civilised natives, or natives who work for white men or live in townships; as many as a third may shew symptoms of the disease, or admit infection.

It may be stated that all the more advanced Bantu tribes, such as the Zulus, the Basutos, the Mxosa, the Baganda, etc., are quite capable of distinguishing syphilis and its primary and secondary symptoms from

* By African Natives I mean Bantu, which is the predominant coloured race in Africa at the present time.

any similar affections, in fact, it is astonishing how intelligent natives will unerringly differentiate the majority of skin eruptions from each other. Where knowledge fails it is in the recognition of the connection between primary sore and tertiary symptoms. Gummas are usually confused with sores or swellings of non-specific origin; on the other hand, if a Bantu says he has had syphilis, the statement can be considered to be correct.

Treatment with mercury and iodides was hardly applicable on a large scale to raw natives, living scattered over a wide district. A course of treatment which had to be continued for a year at least, with regular intervals, does not appeal to the imagination as a miraculous European medicine; many natives are persuaded they can achieve as much with their herbs and charms.

Extensive experience at the Rietfontein Lazaretto in South Africa has, however, proved that anti-syphilitic arsenical preparations can be easily injected in concentrated form to hundreds of cases rapidly, with a very low accidental mortality, and with the well-known prompt, if not always lasting, effects which have been described in the previous paper this evening.

Taking, therefore, into consideration these two important and favourable factors, a facile and reliable method of treatment and native talent in the diagnosis of the earlier stages of syphilis, it appears that renewed attempts to keep an important race poison in check might be attended with some success. The price of the drugs appears, however, to be a serious obstacle. It is possible that anti-syphilitic arsenical preparations will become cheaper in the course of time, but it should not be forgotten that even the raw native is quite willing and capable of paying for any medicine, the properties of which he recognizes. Poor people, in our sense of the word, do not exist in Central Africa or South Africa; there are no natives who do not know when and how they will get their next meal, and lack shelter and a bed. During the prevalence of a famine, circumstances are otherwise, then all suffer; but in years with a normal crop, raw natives are quite able to raise relatively considerable sums for any object they fancy, no doubt also for recognized treatment. I admit there are some qualifications to this statement, but a discussion of the subject would lead me far from the medical standpoint.

It is remarkable that, as a rule, the pagan native does not seem to

suffer from the terrible mutilations due to untreated tertiary syphilis. These mutilations, as you will see, are not unknown in South Africa, whose natives on the whole are partially civilised. Neither does one meet general paralysis or tabes as a result of syphilis, though I have seen euphoria, expansive megalomania, sluggish pupils and progressive dementia in Uganda as a result of trypanosomiasis.

Another feature of native syphilis is the presence of little boys of 7 to 9 years of age among the individuals infected and the outbreak of accumulated contagion in families, some of the cases being due to apparent indirect extra-genital transmission, as shewn by the following example from Bugalla on the Sesse Islands:—

A young girl of 18, from another island, came on a visit to relations; she infected her host and his little son (*æt.* 10 to 12). The former gave the disease to his wife; her little baby developed syphilitic rhagades of the mouth; the elder daughter, a girl about 14, also developed a syphilitic rash; no primary sore could be detected, and sexual intercourse was denied. Five cases all told.

Cases in which a typical secondary rash develops without the presence of a primary sore are met with from time to time.

Abortion appeared to me to be common among the Baganda; how much was due to syphilis is very difficult to state; malaria, spirochætosis, native medicines and customs also play a part in bringing about miscarriages.

There is no gainsaying that, on the whole, syphilis is not as medically important as malaria, sleeping sickness, or other diseases whose prevalence varies a deal in different districts—in any case, as far as the raw native is concerned—still, recent advances in therapeutics render it possible to deal effectively with any localized high ratio of infection, possibly on a self-supporting basis.

The problem how to deal with syphilis among natives working for Europeans on estates or mines is a much more important one, not only because the disease is much more widespread among such natives, but also because it appears incumbent upon the employers or their representatives to look after the workmen's welfare; in addition, there is the danger that native servants may infect by contagion Caucasian children entrusted to their care. In some parts of Africa this is by no means an imaginary danger.

In South Africa, an efficient system of hospitalization and treatment of syphilis has been in operation among the workers in the gold-fields of the Rand for several years, and I believe that it is as successful as any measure which is based on cure only, without the aid of prevention.

Treatment is compulsory, in so far as the men are brought in by the police after having been certified by the mine or district surgeon, but this is an easy task, as the natives are quite willing to be treated. The native workers on the Rand average about 200,000; of these, about 300 to 400 are constantly at the Rietfontein Lazaretto, about eight miles from Johannesburg. The lazaretto has at its head a medical superintendent, with two Caucasian dressers, a manager, a book-keeper, and a matron. The rest of the staff is Bantu.

Originally, treatment used to consist in inunction, and calomel injections; these were later supplemented by one or two intravenous injections of "606" or salvarsan, of which over 8,000 were made. Later, neosalvarsan in concentrated form was adopted, the solution being made with 7 c.c. of warm tap water in the tube containing the drug, aspirated into a small syringe, and injected intravenously on the spot. In this fashion, by preparing the veins beforehand, as many as twenty cases an hour can be injected. The injections are followed by inunction every third or fourth day, and, if considered necessary, calomel is injected intramuscularly. The whole course of treatment does not take over seven weeks, as a rule, though some cases may require a longer period before they can be declared free of any symptoms or danger of recurrence.

Arsenical preparations by themselves have not been found to bring about a lasting cure, and I am demonstrating photographs of a secondary rash developed after injection of 0·6 grammes of "606." The Medical Superintendent of the lazaretto, Dr. M. MEHLISS, considers that though one or two injections of salvarsan are at times not sufficient to cure a syphilitic, yet the drug is an invaluable adjunct, which shortens treatment very considerably, and, moreover, enables one to deal effectively with many bad cases of tertiary and secondary syphilis, which at times reacted but slowly and incompletely to mercury alone. Knowing the rapid action of the arsenical preparation one can await the permanent results of mercurial treatment after the most alarming symptoms have been dealt with.

Accidental mortality, apparently due to the action of the drug on a

debilitated individual, does take place in single and rare instances. It is not unknown even when making use of mercurial injections alone, but in view of the definite and conclusive saving in life, limb and time which follows the use of arsenical preparations, one is certainly justified in taking the risk in the patient's own interest; in the same way one faces the danger of administering an anæsthetic, or, in fact, any time one employs any powerful therapeutic agent.

I mentioned that treatment is compulsory at Rietfontein; accordingly residence in the lazaretto is compulsory till the patient is cured in the opinion of the Medical Superintendent. Such is, however, the faith of the natives in the treatment they receive, that an escape or evasion is an unknown thing, though no special watch is kept and no elaborate fencing is employed. The men are well, if simply, fed, housed in big wooden sheds, and are only lightly occupied in weeding, tree-planting, etc., if at all. I should mention that the Medical Superintendent is a South African, speaks fluently several native languages, so that his local knowledge enables him to maintain order and peace in what might otherwise be a somewhat turbulent crowd, for tribal fights are not unfrequent on the Rand.

In the course of these remarks I have often made allusion to the lack of prophylaxis in dealing with syphilis. I believe that in the course of time the medical profession will have to consider venereal disease among natives from the same standpoint which has so successfully dealt with many other tropical diseases, that is by applying the wise old saw: "Prevention is better than cure."

LIBRARY NOTES.

Under this heading we propose to review recently published works sent us for that purpose, and a short notice of any other works presented to the Library.

Chaulmoogra Oil Treatment of Leprosy.—In Public Health Bulletin No. 75, of the United States Public Health Department, January, 1916,* Dr. GEORGE W. McCOY, Director, United States Leprosy Investigation Station, Hawaii, and Dr. H. T. HOLLMANN, Acting Assistant Surgeon, write on the chaulmoogra treatment of leprosy. They state that the interest that has been shewn recently in the treatment of leprosy by the use of chaulmoogra oil by the hypodermic method has made it desirable to review the reports on the use of the drug, and to record their own experience with it. "According to CASTEL, the hypodermic use of the oil appears to have been proposed first by TOURTOULIS, of Cairo, who, in August, 1899, presented the method before the Academy of Medicine and the Dermatological Society of Paris. He used the oil in this manner for several years before reporting the results, which were favourable but could not be regarded as cures." CASTEL himself did not regard the method as one for routine employment; he reserved it for exceptional cases, at the same time calling attention to its disadvantages, and especially to the danger of fatty embolism (pulmonary), a complication which occurred in two of his cases.

UNNA, in 1900, used the oil both by the mouth and by injections, and claims to have had good results. [It was also used by the latter method at the Seaman's Hospital, London School of Tropical Medicine in the same year.]

ENGEL, who gained his experience in Egypt, agrees with the general opinion that the oil is the most useful agent we have; he recommends giving it in a purified form, known by the name of antileprol. LIE

* Treasury Department: United States Public Health Service. Public Health Bulletin No. 75, January, 1916. Studies upon leprosy, XXVIII.—Chaulmoogra oil treatment of leprosy by GEORGE W. McCOY and HARRY T. HOLLMANN. Washington. Government Printing Office. 1916.

believed that remedy had an undoubted value; he also at the same time mentioned the danger of embolism after hypodermic use. HANSEN, on the other hand, failed to find any beneficial results from it and abandoned it. Others have found that given intramuscularly it causes great pain, and so have stopped giving it by this route. Lately, HEISER, in the Phillipine Islands, reports five apparent recoveries after its use. As regards McCOY and HOLLMANN'S only personal experiences in Hawaii, there is a record of the use of the drug as far back as 1886, and it has probably been used ever since. GOODHUE, in his annual report for 1912, remarked that "chaulmoogra oil was the most popular remedy"; while WAYSON says of it, "chaulmoogra oil still holds a good place in the treatment of leprosy; while no cures have resulted from its use, many cases are greatly benefitted."

The authors give their experience with the hypodermic use of the oil in 42 cases, 16 of which have received injections for periods varying from 10 to 17 months. The results were:—

Improved	... ...	10
Stationary	... ...	4
Progressive	... ...	2

while the remaining 26 had not taken the oil long enough to enable a judgment to be formed. The majority of the cases took the oil by the mouth at the same time that it was given hypodermically, and a number of them had carbon dioxide snow applied locally to the lesions. The dose found most suitable was a weekly injection of 5 cc.m. of a mixture containing 50 per cent. of the oil. Larger doses gave rise to such persistent infiltrations that they had to be discontinued. Some have given the oil alone hypodermically, but it is considered better to combine it in the form of mixtures. Two of these are given in the paper, namely (1) a formula given by JEANSELME, and (2) one devised by MERCADO and used by HEISER, in the Philippines.

(1)	Chaulmoogra oil, which has been washed with alcohol, filtered through cotton and sterilized at 100° C.... ...	1 part.
	Guaiacol, 50 centigrams	1 part.
	Camphor, 50 centigrams	
	Oil of vaseline (vaseline sterilized and filtered), 5 grams	

(2) Chaulmoogra oil ... ... 60 c.cm.
Camphorated oil ... ... 60 ,,
Resorcin ... ... 4 grams.

Mix and dissolve with the aid of heat on a water bath, and then filter.

Complications attending the use of the oil are met with, and the authors had their share of these. Abscesses developed in five cases, three of which required opening and draining, two patients fainted after the injection, and five developed paroxysms of coughing within a minute or two after the completion of the injection. This was complicated in one case by rather severe circulatory depression. In this connection a case mentioned by MANSON in his book on Tropical Diseases (4th edition, p. 562) is referred to; here the patient died from what was suspiciously like fat embolism. [It was fat embolism, the patient, though perfectly healthy that morning, dying some minutes after the injection. At the autopsy, at which the present writer was present, no gross lesions were present that could have accounted for the sudden death.]

McCOY and HOLLMANN'S summary of the subject is as follows:—"Our personal experience leads us to the conclusion that most writers have reached—that the oil is helpful to many cases of leprosy, perhaps the majority.

"The hypodermic method of administration, while not free from disagreeable complications, seems to have given good results, and in view of the nature of the disease ought to be given a further trial. We are sure that the use of chaulmoogra oil as at present practised is not the solution of the problem of the therapeutics of leprosy."

G. C. Low.

TRANSACTIONS

OF THE

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Proceedings of a Meeting of the Society held on Friday, May 19th, 1916, at 5.30 p.m., at 11, Chandos Street, Cavendish Square, W., Surgeon-General Sir DAVID BRUCE, C.B., A.M.S. (*Vice-President*), in the Chair.

CEREBRO-SPINAL FEVER, WITH NOTES OF SOME CASES.

BY

MAJOR J. M. ATKINSON, R.A.M.C.

(Officer in Charge, Richmond Military Hospital).

It occurred to me that a discussion on this disease, which has only become really prevalent in England since the commencement of the War, would prove of interest to the members of this Society.

It is only since the winter of 1914-15 that cerebro-spinal fever has become epidemic in this country.

Prior to this date it was sporadic. This fact is clearly shewn by the Reports of Surgeon-Colonel REECE, one of the Medical Officers of the Local Government Board, who was deputed to collate and report to the Board annually on its prevalence. There were notified in the civil population in England and Wales, during the year 1913, 279 cases of cerebro-spinal fever; in 1914 there came to notice 300 cases. The greatest incidence fell in children under fifteen years of age (80·6 per cent.). The case fatality was high, the mortality rate being 69·6 per cent. in the former years, and, in 1914, 68·7 per cent.

Cerebro-spinal fever is much more prevalent in the United States of America and Canada than in this country, and undoubtedly one of the most important factors that contributed to its prevalence in an epidemic form in the British Isles was the concentration of troops at various centres in this country, and more especially the arrival of the Canadian troops—this was undoubtedly an important factor in the subsequent prevalence of the disease.

To most members of our profession, prior to 1914-15, this disease was only known by name.

DEFINITION.

Cerebro-spinal fever may be defined as a specific disease due to infection of the organism by the *Diplococcus meningitidis* of WEICHSELBAUM, occurring both in epidemic and in sporadic form, and most often shewing itself as a meningitis involving the whole cerebro-spinal axis.

HISTORY.

The first authentic account of an epidemic is that which occurred in Geneva in 1805. Glimpses of the disease, in this epidemic form, are met with in the literature of the Middle Ages.

The epidemic in Geneva does not appear to have been widespread, as only thirty-three persons died of the disease.

The interest lies in the contemporary records: the clinical symptoms were accurately described by VIEUSSEUX, as well as the post-mortem appearances by MATTHEY.

VIEUSSEUX writes: "The initial symptom was a sudden failure of strength; the expression was anxious; the pulse was feeble, sometimes threadlike, in a few cases hard and bounding. There was violent headache—in the main, frontal.

"The headache was followed by the vomiting of green matter, by stiffness of the spine, and, in infants, by convulsions.

"The body shewed livid patches after death—occasionally, during life."

MATTHEY well describes the post-mortem appearances: "The vessels of the meninges," he says, "were notably congested; a gelatinous humour covering the brain was markedly tinged with blood. There was fluid in the ventricles. The choroid plexus was of a deep-red colour.

"The base of the brain was covered by yellow puriform matter, with no obvious change in the underlying cerebral tissue. The exudation covered the optic chiasma and extended backwards towards the cerebellum, reaching for the space of an inch down the vertebral canal."

The subsequent history of the disease is well described in FOSTER and GASKELL'S monograph published this year.

Read in the light of our present knowledge, the part played by the carrier in the spread of the disease affords a clear explanation of the records of these long past epidemics.

Assuming the presence of a few permanent carriers, it only requires outside conditions which facilitate the spread of the organisms to create a large number of temporary carriers, in other words, "the number of the carriers constitutes the epidemic."

The persons who fall sick of the disease are thus but the concrete evidence of the wide diffusion of temporary carriers.

The disease spread to Canada in 1807, to Virginia, Kentucky and Ohio in 1808, appearing in "the State of New York" and in Pennsylvania in 1809.

This American epidemic was remarkable in the giving of it the name of "spotted fever," by which name it is described in a book entitled, *Treatise on a Malignant Epidemic called Spotted Fever*, written by NORTH in 1811.

During this period the disease was rife in Europe also.

In 1806-7 it broke out in the Prussian Army, amongst the Spanish prisoners at Brianca, at Brest in 1813, and at Paris in 1814. These epidemics coincided with several outbreaks of typhus fever, with which the cases of cerebro-spinal fever were often confounded.

Between 1837-1850 a very wide-spread epidemic appeared in France, the disease being carried from place to place by the transfer of troops.

The years 1851-61 included a large epidemic in Norway and Sweden; in the years during which the disease raged it caused no less than 4,138 deaths in Sweden, where only two provinces out of twenty-four, and those the most northerly, escaped.

Coming down to recent years (1905-15), there has been a revival of the disease in an epidemic form over a very large area. This revival of cases appeared to have started by the large outbreak which occurred in the United States in 1904.

New York, in the year 1905, had no fewer than 2,755 cases. Two years later, an almost equally severe outbreak occurred at Glasgow, at Edinburgh, and at Belfast.

The New York epidemic gave FLEXNER full scope for trial of the serum manufactured according to his method and tested on monkeys.

This period in the history of the disease produced important bacteriological researches, establishing beyond reasonable doubt the causative nature of WEICHSELBAUM'S coccus, and completing our knowledge of its cultural and bio-chemical, characters.

The same period also saw the evolution of the doctrine of the carrier, a doctrine which affords the most satisfactory solution of the problem of the dissemination of the disease.

The most recent experiences of the disease are that of the present epidemics in England and in France. At various barracks and camps the disease has appeared, and, as usual, the armies in the field have almost entirely escaped.

Civilians have also suffered, a good deal of evidence going to shew that some of the soldiers have acted as carriers whilst in billets, and when on leave.

The coincidence of severe outbreaks of influenza have contributed an additional catarrhal factor in connection with the epidemiology of the present outbreak of cerebro-spinal fever.

It is principally a disease of towns. Surgeon-Colonel REECE in his notes on the prevalence of cerebro-spinal fever during the last four months of 1914, and the first six months of 1915 *(Journal of the Royal Army Medical Corps for June, 1915)*, gives the percentages as follows:—

Urban Districts	66·3.
Rural Districts	10·3.
London	23·3.

It is protean in nature. The period of incidence decreases with advancing age.

From the same paper I find that the first military case in 1914 was that of a Somerset Yeoman at Great Bentley, in Essex. The first symptoms of illness occurred on 19th September, 1914, he went home to Taunton on 23rd September, and died there on October 14th.

The next, of 18th October, 1914, was a soldier of the Canadian contingent in the Bustard Camp on Salisbury Plain. There had been

four cases of cerebro-spinal fever amongst the Canadian troops while in Canada and before embarkation; three cases occurred during the voyage, two, a soldier and a steward's clerk on a transport, and one soldier on another.

This case came from a transport on board of which no case occurred during the voyage. Three more cases occurred amongst the Canadians on the Plain on the following dates respectively—27th October, 30th October, and the 4th November, 1914.

Between January 9th, 1915, and July 31st, 1915, there was quite an epidemic, the following being the number of cases that were notified:—Civilians, 2,290; Troops, 1,088; the highest incidence being in the week ending March 13th, 1915, the numbers notified for that week being:—Civilians, 147; Troops, 87.

SYMPTOMS.

Incubation.—The period of incubation varies from three to five days. The former limit would appear to be the more usual period, while in the malignant forms this may be still further shortened. The number of cases on which it may be gauged are few.

Invasion.—The onset is, as a rule, sudden, frequently being ushered in by a rigor, then follow the usual symptoms of an acute specific fever, viz., a temperature of 102° F. to 104° F., usually rising rapidly, and attaining to a fairly high degree on the first day.

A short period of general malaise is succeeded by headache, which is often intense. A striking feature in many cases is complete loss of appetite, amounting even to absolute revulsion against any kind of food; this is frequently followed by vomiting, then follow pains in the neck and limbs, retraction of the head, and some degree of catarrh of the nose, throat, eye or ear.

In severe cases delirium soon supervenes, followed by coma and death. There is a peculiar distinctive cry about this delirium.

The height of the temperature forms no criterion of the severity of the disease, some of the most rapidly fatal cases shewing but a very slight rise. The pulse is usually quickened, but as a rule not to the extent which would be expected from the temperature. The occurrence of a pulse of 60 to 80 accompanying a temperature of 101° F. to 103° F.

is not uncommon in the early stages of the disease, and is of considerable diagnostic importance.

Eruption Stage.—This usually appears during the first week and may be macular, erythematous, petechial or purpuric.

The maculæ do not come out in successive crops, begin to fade rapidly, and disappear usually in four days.

The erythematous rash is like the transient erythema which may precede the eruption in small pox or typhus. Like the maculæ they may appear at any part of the body.

When petechiæ and a purpuric rash appear, they are evidence of profound toxæmia.

Herpes is common. There is usually some leucocytosis—20,000 to 40,000 per c.mm. Kernig's sign is present; this occurs in all but the fulminant cases, it is one of the earliest symptoms to appear. The pupils are usually dilated, they may be unequal. Towards the end of the first week a condition of semi-stupor occurs, accompanied by marked retraction of the head. The temperature remains fairly high and approaches to the continued type.

The patient remains quite the same for two or three weeks and recovery is very gradual; ultimately the fever completely subsides, the headache and stupor pass off and the rigidities shortly disappear; the actual convalescent stage once being established is rarely interrupted, it may be weeks before the patient is free from stiffness. Unfortunately paralysis of single muscles, or of groups of muscles, may remain. Permanent deafness and blindness are by no means uncommon, and in some cases the patient never recovers his mental power and remains an imbecile.

DIAGNOSIS.

WEICHSELBAUM discovered the meningococcus in 1887; QUINCKE introduced lumbar puncture in 1890. It is the presence of the diplococcus of WEICHSELBAUM in the cerebro-spinal fluid that establishes the diagnosis.

The disease with which it is most often mistaken is influenza; fever prolonged past the seventh day, in the absence of any complication is unlikely to be due to influenza.

There is not time to go into the differential diagnosis. However,

I would mention that in typhoid the headache usually ceases when delirium commences, but in this disease it is not so.

BACTERIOLOGY.

It is the presence of WEICHSELBAUM'S gram-negative diplococcus in the cerebro-spinal fluid, obtained by lumbar puncture, that clenches the diagnosis, and fulfils all KOCH'S postulates; it can be grown outside the body in pure culture, it is capable of producing the disease when injected into susceptible animals, and it can be recovered from the lesions so produced and again cultivated outside the body.

During the first fortnight of the disease the meningococcus can usually be recovered from the naso-pharynx.

The organisms are generally almost entirely intra-cellular, lying within the bodies of the polymorphonuclear leucocytes; but there are exceptions; they may be more numerous in the fluid. The number of cocci in the cerebro-spinal fluid varies very much in different cases, and also in individual cases in the stage of the disease; though earlier the disease as a rule, the fewer the meningococci.

The differential characters of the meningococcus are :—

(a) The majority of strains of meningococci fail to grow on ordinary media at a temperature below 25° C.

(b) The meningococcus ferments glucose and maltose with the production of an acid reaction, but fails to change saccharose.

(c) The meningococcus shews positive agglutination with homologous antimeningococcal serum, and has the capacity of removing or absorbing the specific agglutinin from such serum.

The last appears to be the most valuable of available positive tests for identifying the meningococcus. It is necessary, however, to do a control test with normal serum, and also with a suspension of homologous meningococci.

The specific agglutinating serum used for testing suspect meningococci from the naso-pharynx of contacts should be of proved activity, not only against strains of meningococcus from the same outbreak, but also against strains from the cerebro-spinal fluid of recent cases of cerebro-spinal fever in the district where the disease is manifesting itself.

Lieut.-Col. MERVYN GORDON has shewn that in the present epidemic there are four or more distinct strains of virulent meningococci.

TREATMENT.

Antimeningococcal serum should be injected intraspinally after the removal of an equal amount of cerebro-spinal fluid. The serum should be warmed to the body temperature, at least 20 c.c. injected, and the foot of the bed should be raised after the injection. The dose should be repeated daily if necessary. The indication for further serum should be the turbidity of the fluid drawn off; if it is still turbid it is well to give more serum. Intercurrent conditions should be treated as they arise, special care being taken of the eyes. The indication for further serum is proportionate to the number of the meningococci still present, as found by examining a film of the withdrawn cerebro-spinal fluid.

Many aver that mere lumbar puncture without the injection of any serum is, in many cases, curative. Should the disease be met with sufficiently early, treatment by daily lumbar puncture would hold out considerable hope of success. Our failure, in the first months of the present epidemic, to secure the excellent results attained by serum injection in America was apparently due to the fact that we relied largely on sera obtained from American sources; whereas the types of meningococci prevailing in this epidemic differed from those found in the United States.

MORTALITY.

The mortality in cerebro-spinal fever is undoubtedly higher in the epidemic than in the sporadic cases. In epidemics, the mortality prior to the introduction of serum treatment was about 70 to 80 per cent. Since the serum treatment, the mortality in epidemics has fallen to about 30 to 40 per cent.

CARRIERS.

Carriers have been already discussed, but the spread of an epidemic of cerebro-spinal fever is not explained by the discovery of the coccus in the naso-pharynx of *actual* patients, for disease rarely spreads by direct contact from patient to patient.

This difficulty was solved when ALBRECHT and GHON found that the

coccus could be isolated from the naso-pharnyx of healthy persons living in epidemic zones.

In other words, they isolated the meningococcus from healthy contacts; for every case of cerebro-spinal fever there exist in times of epidemic a number of healthy carriers variously estimated at from ten to thirty.

The number of contacts varies with several inconstant factors:—

1. There is a seasonal incidence for carriers as there is for cases; there are twice as many carriers in the spring months as in the summer months.

2. The number of carriers varies also with the stage, and with the character of the epidemic; there are more carriers when the epidemic is fully developed than during the waxing and waning stages, and there are also more carriers when the epidemic is severe than when it is mild.

3. The closeness of contact between the individuals affects the number of carriers: mothers who nurse their meningotic infants are found to be carriers almost invariably, and carriers are particularly abundant in schools and barracks.

The conditions under which the meningococcus is carried in the throat are very similar to those of diphtheria carriers where also the organisms' stay in the throat may be temporary or prolonged.

In diphtheria the organism may exist in some for years.

The limit with the meningococcus as far as it is at present known is six months. Surgeon-Colonel REECE tells me of one case in which it has lasted for a year, and another case, like "Charley's Aunt," is still running.

A temporary carrier is comparatively easy to deal with. The organism seldom survives for as long as a fortnight in the throat; contacts giving two negative results may be discharged to duty.

In a number of cases the first swab is positive, while the second swab, taken some ten days later, and all subsequent swabs, proved negative. The period of isolation required in such a case is, therefore, short.

Prolonged carriers are very much more difficult to deal with, as it is hardly practicable even in the case of troops to isolate an individual for

as long as six months; this problem is still without a satisfactory solution.

TECHNIQUE OF PASSING THE NASO-PHARYNGEAL SWABS.

Two matters are of prime importance in this connection :—

1. The swab should be as free as possible of contamination with saliva.

2. As the meningococcus readily dies on the swab, it is desirable to make the cultures as quickly as possible, and for these cultures to be got into the incubator as quickly as possible.

With a view to avoidance of contamination by saliva, a special covered swab such as that introduced by WEST is suitable. The man is seated facing the light. His mouth is opened as wide as possible, and the tongue depressed with a spatula. During phonation the covering tube containing the swab is passed behind the soft palate, and the swab extruded so as to come in contact with the naso-pharynx. The swab is then drawn back into the covering tube and the instrument removed.

PUBLIC HEALTH PRECAUTIONS.

Prevention.—The meningococcus is spread by being carried in the nose and throat of normal individuals, convalescent patients, and patients actually suffering from the disease, and is imparted to other individuals by direct contagion.

Such contagion takes place either by actual contact, such as kissing, or by spraying of the discharges of the throat and nose, as in coughing, speaking, singing and sneezing. The question of preventive measures, therefore, resolves itself into the isolation and treatment of such persons. The average period of time during which a person may carry the active infection in his naso-pharynx is twenty-one days; a fairly trustworthy result should be available in four or five days; this is a very important result as regards military efficiency, as it means a saving of seventeen days for each contact who has been found positive.

There is also a possibility that the organism might be spread by the contamination of articles of clothing, or of walls and floors by such proceedings as spitting, but the extreme susceptibility of the meningococcus to drying renders such method of spread at least very unlikely. The

utensils used in food, such as cups, spoons, etc., may undoubtedly spread the infection.

The fact that by far the greater number of carriers have normal throats, therefore no excessive discharge from the nose or posterior naso-pharynx, and thus little inclination to indulge in spitting, is an additional factor in making such a method of spread improbable.

The treatment of convalescent cases, as regards the possibility of their spreading infection, is similar to that of carriers.

There is no reason why positive contacts should not be allowed to take exercise in the open air, provided they are isolated from their fellow-creatures, and care is taken to mark their food utensils.

Prophylaxis.—Prophylaxis for the healthy individual resolves itself into the use of general hygienic measures, especially avoiding cold, catarrhal affections and overcrowding.

In epidemic areas, schools should close, and troops leave barracks for roomy huts or tents in the open air; provided in each case there is no incidence of the disease nor occurrence of carriers amongst the individuals concerned. The billeting of soldiers upon civilians should cease.

Notes of Bacteriologist *re* the Richmond Military Hospital Cases.

(For these Notes, I am indebted to Captain M. W. Flack, R.A.M.C., Bacteriologist at Millbank.)

In the diagnosis of the following four cases, and in the bacteriological examination of their contacts, the Millbank medium, consisting of trypsinised ox heart broth, to which agar and an extract of pea-flour are subsequently added, has been used throughout—before pouring, 2 per cent. of ordinary horse serum is added. When endeavouring to grow the meningococcus from the cerebro-spinal fluid of the patient, a little fresh human blood is spread over the surface of the medium.

The examination of contacts is performed by swabbing their naso-pharynx (using a West's swab) and plating the swab *immediately*. The plates are incubated for 24 hours, and suspicious colonies of gram-negative cocci are subcultured on plates. These are incubated for another 24 hours.

The resulting growth is washed off and emulsions prepared with saline, and, in order to inactivate the antolysin, are heated for half an hour at a temperature of 65° C. They are then standardised to a strength of 2,000 million per c.c. and 5 per cent. of phenol added; with the predominate types of meningococci of last year's epidemic, high titre agglutinating sera have been prepared. The emulsions are tested against these, and, if necessary, the result can be confirmed by the absorption of agglutinin.

Case 1.—Pte. X., Y Regt., came in from parade on Friday, December 13th, 1915, complaining that he felt queer and went to bed early. During the night he became delirious, was very violent, and vomited occasionally. He was transferred from his billet to Richmond Military Hospital at about 1 a.m., December 18th. Just before death, lumbar puncture was performed, and a turbid purulent fluid was drawn off.

Microscopically the fluid shewed masses of polymorphonuclear leucocytes and numerous gram-negative cocci. both extra-cellular and a few intra-cellular. Culturally the fluid yielded an abundant growth of gram-negative cocci. An emulsion was prepared and the coccus submitted to the agglutination test. The organism proved to be a Type I. meningococcus, which result was subsequently confirmed by absorption.

Four contacts from the billet, three intimate friends of deceased and two R.A.M.C. orderlies from Richmond Military Hospital were swabbed. These were all found to be negative. From four swabs suspicious gram-negative cocci were obtained, which the agglutination shewed to be negative.

Case 2.—Driv. X., Y Regt., was sent to the Richmond Military Hospital on February 19th, 1916, suffering from what was suspected to be acute rheumatism. He complained of severe pains in head and bottom of back, accompanied by vomiting. On February 22nd, cerebro-spinal fever was suspected and he was isolated. Lumbar puncture was performed and turbid cerebro-spinal fluid was removed. Microscopically the fluid shewed many polymorphonuclear leucocytes and both extra- and intra-cellular gram-negative cocci. On blood plates a growth of meningococcus was obtained, which was shewn by agglutination to be of Type II. See Table I. A coccus of the same type was isolated from his naso-pharynx.

After diagnosis the man was removed to Fulham Military Hospital,

where he was given several doses of the new military anti-meningococcal serum, prepared at the Lister Institute from the types of meningococci, prevalent in last year's epidemic. The patient reacted well to the serum, and he recovered rapidly and completely. Also his naso-pharynx cleared up speedily, and he was declared negative on April 6th, 1916, and was discharged from Fulham Military Hospital to a Convalescent Hospital.

At Burvale House, Hersham, where the patient was stationed, eighteen contacts were examined, mainly those who occupied the same bedroom. The R.A.M.C. orderly, who attended this man from the time he was taken ill until he was transferred to Richmond, was included. Of these eighteen, twelve were declared negative on first examination of the plates, and six suspicious cocci were subcultured for agglutination. As the attached Table I. shews, they were positive and of the same type meningococcus as the case.

At Richmond Military Hospital, of twelve contacts examined, three shewed suspicious gram-negative cocci, which were subsequently raised for agglutination. One proved to be positive and of the same type as that of the case.

Of the three positive contacts at Burvale House, one slept in the next bed, one exactly opposite, and the third—the R.A.M.C. orderly who attended him—in the same room.

The positive contact at Richmond Military Hospital was in the same ward and in attendance on the patient. The four positive contacts were transferred to carrier wards. The R.A.M.C. orderly cleared up almost immediately; one Burvale House carrier was discharged after two negative swabs on April 18th; the other Burvale House carrier is still in quarantine (end of April); and the Richmond contact was discharged on April 21st after two negative swabs.

Case 3.—2nd A.M. X., Y Regt., stationed at Rochampton, came from Larkhill Camp, Salisbury, on Saturday, 11th March, and, at 7 p.m. on the same day, was suddenly taken ill with excruciating pains in head and back, and vomited on Sunday morning, when he was admitted to Richmond Military Hospital. His temperature on admission was 102° F.* He quickly became delirious and very violent, coma supervening. He died at 2 p.m., soon after lumbar puncture. The fluid drawn off was

* At Larkhill Camp there was a batch of men who had recently arrived from The Curragh, where cerebro-spinal fever was prevalent.

turbid, and shewed microscopically many gram-negative cocci extra- and intra-cellularly. An abundant growth was obtained, and the organism was proved by agglutination to be a Type IV. meningococcus. In all, twenty-nine contacts were examined. These consisted of nineteen men who had slept in the same hut at Roehampton for one night only; the N.C.O. in charge of the hut; five men who had travelled in the same railway carriages as the patient, either from Larkhill to Waterloo or from Waterloo to Barnes; one man who had slept in same hut at Larkhill; and three orderlies from Richmond Military Hospital. Three shewed suspicious cocci, which were submitted to the agglutination test. Two were positive, but only one of the same type as the case, and it is interesting to note that the one true contact was the man who slept in the same hut at Larkhill Camp. The other positive contact must be looked upon as an accidentally found carrier. See Table II. for results.

Case 4.—Pte. Y., E Regt., was admitted into Richmond Military Hospital on March 30th, 1916, and was transferred to Fulham Military Hospital on the same day. He had spent the previous seven days in detention, and before that was on leave, and had slept for one night only in hut 44 at Wimbledon. He complained of intense headache and stiffness of the neck, and of pains practically all over the body more like those of acute rheumatism. His temperature was 102° F., and he also vomited at times. Lumbar puncture was performed and a clear fluid drawn off, which shewed, on examining the centrifugalised deposit, no cocci and few cells; and, in spite of many subsequent endeavours, it has been found impossible to grow it—no organism could be grown. From his naso-pharynx, however, a suspicious organism was isolated, and proved, after agglutination, to be a meningococcus of Type II. The man was, therefore, regarded as a case; this is confirmed by the marked response to serum treatment. Twenty-three contacts were examined; of these, three shewed suspicious gram-negative cocci, which were raised for agglutination. One proved to be positive and of the same type as the case.

Clinical Notes.

Case 1.—Admitted Richmond Military Hospital, 18th December, 1915, complaining of severe headache; vomiting. Temperature on admission, 102·6°; pulse only 68.

Kernig's sign present. Lumbar puncture the same day and 20 c.c. of anti-meningococcal serum injected intraspinally.

Fluid turbid. Very somnolent. Became comatose that evening, and died December 19th, 1915.

Case 2.—Admitted 23rd February, 1916, from Richmond. Headache, slight head rigidity, slight Brudzinski, slight Kernig, rational.

Temperature 100°, pupils dilated, vomiting lumbar puncture at Richmond on 22nd February, 1916.

Lumbar puncture repeated on 24th, 25th and 26th, followed on each of these occasions by administration intrathecally of 30 c.c. of anti-meningococcal serum (Lister).

From the 26th there was steady improvement, and he was discharged to convalescent home on 10th April, 1916.

Case 3.—Admitted to Richmond Military Hospital, 12th March, 1916—on admission, temperature was 102·6° F.—complaining of intense headache with pains in all his limbs. Kernig's sign present. Only came up from Salisbury yesterday; began to feel unwell the same evening.

Was placed in the isolation ward; in a short time became quite maniacal, so much so that it was difficult to restrain him. He had the peculiar cry which I have mentioned before as ushering in the delirium.

The delirium merged into coma, and he succumbed at 2.10 p.m. the same day.

He was lumbar punctured at 1.15 p.m., and 30 c.c. anti-meningococcal serum was injected intraspinally.

Case 4.—Admitted from Wimbledon on 30th March, 1916. Pains all over, severe headache, head rigidity, Kernig's sign, photophobia. Temperature 100°.

Lumbar puncture with administration of Lister serum on 30th and 31st March and April 1st.

On April 1st. Vomiting, semi-conscious, incontinent.

On April 3rd. Great improvement, disappearance of all meningitic signs.

May 3rd. Perfectly well, except that postnasal swab still positive; meningococci present.

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On April 1st. Vomiting, semi-conscious, incontinent.

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May 3rd. Perfectly well, except that postnasal swab still positive; meningococci present.

TABLE I.—Sera.

Cocci.	N.S.	I. Serum.			N.S.	II. Serum.			
	1/10	1/10	2/10	4/10	1/10	1/10	2/10	4/10	
Standard Sera *v.* Homologous Cocci	−	+ +	+ +	+ +	−	+ +	+ +	+ +	Controls.
Moore C.S. Fluid ...	−	−		−		+ +	+ +	+ +	Positive (II).
Burvale 5 . ..	−	−	−	−		(+)	−	−	Negative.
,, 6	−	−		−		−		−	Negative.
,, 8		−	−	−		−		−	Negative.
,, 9		−	−	−		+ +	+ +	+	Positive (II).
,, 10	−	−	−	−		+ +	+ +	+ +	Positive (II).
,, 12	−	−	−	−		+ +	+ +	+	Positive (II).
Richmond 8... ...	−	−	−	−		−	−	−	Negative.
,, 9... ...		−	−	−		+ +	+ +	+	Positive (II).
Nurse R	−	(+)	−	−			−	−	Negative.
Moore N.P.... ...	−		−	−		+ +	+ +	+ +	Positive (II).

TABLE II.—Sera.

Cocci.	N.S.	I. Serum.			N.S.	II. Serum.			N.S.	III. Serum.			
	1/10	1/10	2/10	4/10	1/10	1/10	2/10	4/10	1/10	1/10	2/10	4/10	
Standard Sera *v.* Homologous Cocci		+ + +	+ +	+	-	+ +	+ +	+ +	-	+ +	+ +	+ +	Controls.
Myers C.S. Fluid ...		−	−			(+)	−			+ +	+ +	+	Positive (IV).
Bloomfield	−			−		+ +	+ +	+ +			−	−	Positive (II).
Carver	−	−	−				−	−		+ +	+ +	(+)	Positive (IV).
Strive	+ +	+ + +	+	+ +		+ +	+ +	+ +		+ +	+ +	+ +	*M. Flavus.*

Note.—N.S. = Normal Serum. N.P. = Naso-Pharynx. It has been shewn that cocci, which agglutinate with normal serum, are *M. Flavus.*

TABLE III.—Sera.

Cocci.	N.S.	I. Serum.			N.S.	II. Serum.			N.S.	III. Serum.			
	1/100	1/100	1/200	1/400	1/100	1/100	1/200	1/400	1/100	1/100	1/200	1/400	
Standard Sera *v.* Homologous Cocci	−	+ +	+ +	+ +		+ +	+ +	+ +		+ +	+ +	+ +	Controls.
Nay N.P.	−	−	−	−		+ +	+ +	+				−	Positive (II).
P.B. 6...	−	−	−			−	−						Negative.
,, 7	−	−	−				−	−			−		Negative.
,, 12...	−	−		−		+ +	+ +	+					Positive (II).

Note.—N.S = Normal Serum. N.P. = Naso-pharynx. It has been shown that cocci, which agglutinate with normal serum, are *M. Flavus*.

Fleet-Surgeon P. W. BASSETT-SMITH: As no one else rises, I should like to open the discussion. In the first place I think the question of the bacteriology of cerebro-spinal fever is not quite so plain as is often considered. We know from fact and experience that there are cases where no organism is found in the cerebro-spinal fluid; thus you have cases clinically indistinguishable from cerebro-spinal fever, in which bacteriologically you cannot prove the disease. I only point that out for what it is worth. Of course, that point has been taken strongly hold of by HORT and others. I think at present one ought to have an open mind from the bacteriological point of view on this question. No doubt it seems so plain: you have got your organism which is infective; you have that organism in the cerebro-spinal fluid, blood, throat, etc.; the organisms may be injected into animals, and can be recovered. These are important points; but there is another one: you can get the organism from the throat of the patient suffering with the disease and yet it is apparently not infective. Why is it not infective? At the same time you get from the throat of another person who is quite well, a similar organism, and it is infective. There are many points to be considered besides the organism itself.

Then with regard to carriers. These carriers, when they are prevalent in the different stages of the epidemic, and in different periods of the year, are not, I think, quite so constant as stated. Carriers vary enormously with the character of the people and the health of the individuals. Take for instance a lot of patients at the London hospitals. If you take a number of contacts from these you get a comparatively high number of carriers. You take a number of very healthy men, for instance in a depot for which I have been lately working, where the number of contacts is very low. It depends a great deal on the health and stamina of the people, and more especially on the age of the people. That plays a very important part. For instance, in the London Hospital there are 10 to 20 per cent. of carriers; that calculation was carried out by a high-class bacteriologist. Speaking with a large experience, out of 4,000 cases, my percentage is only 1·3. Every man before being drafted away had to be examined to see whether it was safe or not, and the percentage worked out at 1·4, so that the health of the person, the conditions under which he lives, and his age, are most important points with regard to carriers.

Then another point. Major ATKINSON said he would let a man go after four days with two negative swabs. In the first place I think it is a difficult thing to get the result of the first swab out on the fourth day, and it would take the same period, or longer, to get out the second, so it would not be until about ten days. In the bacteriological description it is stated that the culture is made from the throat direct on to the plate, and grown 24 hours and sub-cultured, and the agglutination tests applied. There is no mention of any fermentation reactions whatever. I should not like to take any positive opinion of any case upon that.

Then another point. Major ATKINSON referred to FLEXNER serum. In a letter I received he (FLEXNER) said the serum was not good, but he thought that was because on the market there was some American serum which was old and was not reliable, and as there has not been an epidemic in America for some time, the serum was consequently not so powerfully anti-toxic as it should have been. This year he made fresh serum, which he supplied to us for use, and that serum has been more favourable than any other we have used.

Major TOOGOOD: Following the remarks of the last speaker, perhaps I may be allowed to mention an epidemic, a small localised outbreak which occurred in the south-east of London. Clinically, all these cases were quite indistinguishable from cerebro-spinal fever of the meningococcus origin, but the organism which was isolated both from the throat and also from the cerebro-spinal fluid was a short bacillus of the zeroid type. As I say, clinically speaking, the outbreak exactly resembled that of cerebro-spinal fever, and yet the organism appeared to be entirely different.

Dr. HARRINGTON SAINSBURY: I think that during the prevalence of an epidemic of this type it is very important that we should investigate all cases of headache that persist. The second case Major ATKINSON referred to was under my care at the Military Hospital, Richmond, and I was puzzled over it. The patient did not seem particularly ill; there were no rigidities in neck or arms; no rash. I did lumbar puncture and obtained a fluid which looked rather doubtful, but which the bacteriologist's report shewed to be definitely meningococcic in nature. We thought the patient should make a good recovery, and this he did.

In cases of this kind which we clinically suspect, I do not think we should lay too much stress on the negative findings of the bacteriologist; I mean as to the presence of the meningococcus, for we are familiar in other forms of disease—definite tuberculosis, for instance—with negative results, although we know the disease to be present. Accordingly, if in doubt, we should proceed in our treatment upon the clinical findings. And here it is important to remember that simple tapping gives good results with a minimum of risk. I believe Major MICHAEL FOSTER, of Cambridge, treated most of his cases with tapping simply. However, though convinced that we have in tapping a very powerful means of combating the disease, I should always have recourse to serum if I could get it.

From another point of view, in regard to treatment, we should, in my opinion, proceed on clinical results rather than on bacteriological. I remember a case at the Royal Free Hospital, a sporadic case, in which the organism was found to be a pneumococcus. The patient, a boy, did extremely well on serum treatment, and the trouble appeared to be subsiding satisfactorily; then, for some reason, I did a tapping, and, to my surprise, found that the organism was still present. On the strength of this I gave a further injection of serum with rather bad results, suggestive of anaphylaxis; he had a definite set back. This made me decide, in this particular case, to proceed thenceforth on clinical lines only. The boy ultimately made a good recovery.

The question of the isolation of carriers of the meningococcus raises many difficulties, and I think we may have to be content to go upon reasonable rather than upon strictly scientific lines. It is not possible to isolate diphtheria carriers for the weeks and months of the persistence of the bacilli. It is not practical politics to do so. I think the same question arises with regard to cerebro-spinal fever, the germ of which may continue for months in the apparent presence of complete health.

Dr. F. HEWKLEY: May I ask if local treatment is of any efficacy, or, if the meningococcus cannot be isolated in the cerebro-spinal fluid, and a positive result obtained by examination of the pharynx, would local treatment by the use of the serum be at all expedient?

Major J. M. ATKINSON: There is no doubt that, with contacts, spray-

ing the throat, using a gargle of 1/1000 permanganate in 1·5 per cent. solution sodium sulph., will cause dispersion of the meningococci from the pharynx; also spraying the posterior naso-pharynx with a preparation of menthol, grs. viii.; iodine, grs. iv.; purolein, ad. ʒi.

Surgeon-General Sir DAVID BRUCE: Then you have no knowledge of the use of serum locally?

Dr. R. A. O'BRIEN (in response to the President's summons): I am afraid the only information I can supply bears on this very depressing question of the inefficiency of the serum we had in the early part of last year. From clinical accounts, I think there is no doubt that none of the sera available for treatment was of very great use. That was rather borne out by the examination of the serum in the laboratory. I do not know whether the strain of meningococcus occurring in England changed in any way. It looks as though they had, and yet when one compares them, one finds that most of the strains obtained to-day do correspond with the strains worked with years ago, *i.e.*, meningococci and para-meningococci. People who were making serum had the original strains of coccus, and they were immunising their horses with them; but it nevertheless remains a fact that all the serum available in the early part of 1915 had very little antibody reaction to strains isolated at that time. To-day it is all altered. Those in England who are making serum have obtained many strains from cases occurring on the Continent and in England, and are using them. Dr. FLEXNER very kindly sent me over some of his latest serum, obtained about the middle of the year, and it had exceptionally high content of antibodies. He had English strains sent over to America, and I believe that he found the earlier New York serum had a very low antibody content against such strains. Serum now obtainable in England has very high antibody content, and this is rather borne out by the fact that favourable clinical results have been reported from a number of sera now being used.

Surgeon-General Sir DAVID BRUCE: The Society owes a debt of gratitude to Major ATKINSON for his excellent description of the meningococcus. As far as my second-hand knowledge goes, his paper gives fully and accurately the facts as known up to the present day.

The question has arisen in the course of the discussion as to whether this coccus is the *causa vera* of cerebro-spinal meningitis. As far as one can see at present, there can be little or no doubt of this. It is true that cocci are a dangerous and elusive race, but this species is found so constantly in the cerebro-spinal fluid of cases of cerebro-spinal meningitis, that most workers agree that it must be the real begetter and originator of this disease. A fact which seems to me to carry a good deal of weight in this connection is, that when a particular strain or variety of the meningococcus is found in a particular epidemic, the same strain or variety is also found in the contacts. Some one has said "that no truth lasts longer than fifteen years," but I dare to prophesy that the life of this coccus will be longer.

Major ATKINSON told us that in regard to the relation of the meningococcus to this disease, KOCH'S three postulates have been fully satisfied. What about the third? At present the only way of setting up the disease in healthy animals by means of a pure culture is by injecting it into the cerebro-spinal canal, but many other organisms, if injected the same way, give rise to a similar inflammation. Until the disease is brought about by implanting the meningococcus on the mucous membrane of the pharynx, it can hardly be said that all three of KOCH'S demands have been fully answered in the affirmative.

In regard to vaccination against cerebro-spinal fever by the injection of the meningococcus, little has been done up to the present. Dr. PENFOLD, of the Lister Institute, carried out a series of inoculations at Salisbury, during the epidemic which occurred there a year ago, with what is reported to have been results suggestive of success. Lately, another opportunity occurred of testing the benefit of vaccination in a camp of Colonial troops, but Colonel GORDON informs me that although the epidemic ceased, there was no proof that this was due to the vaccine, as all the soldiers in camp were inoculated. There were no controls by which to test the results. On the whole, I think that the prospect of prophylactic inoculation against cerebro-spinal fever is favourable.

Lastly, as regards serum therapy. According to Colonel GORDON there are four strains of the meningococcus at present in England causing epidemics of cerebro-spinal fever. At one place an epidemic is caused by one strain, at another place by a different one. It then is necessary, in order to get good results, that a polyvalent serum got from horses,

immunised against all the strains, should be used. When it is known what strain is responsible for a particular epidemic, then, of course, a monovalent strain can be used. No statistics are as yet available, but I am informed that the treatment of cases by the polyvalent serum supplied by the Lister Institute has been highly successful.

Major J. M. ATKINSON: I thank you very much for your kind remarks on this paper. With reference to Fleet-Surgeon BASSETT SMITH'S remarks, he asks, "How is it that people suffering from the disease themselves are not more infective." I think the reason is that the meningococcus in the naso-pharynx is so easily contaminated by contact with the saliva. It is difficult to understand the meningococcus coming from the pharynx without being contaminated by the saliva. In most cases of spitting, and even of kissing, contact with the saliva renders it in a very short time practically innocuous. This is the reason why they use West's swab in examining the nasal pharynx of contacts.

With regard to the four days, after finding two negative results, the contacts may be discharged to duty. A fairly trustworthy result should be available in four or five days.

I think one important point in the clinical history is the comparative slowness of the pulse with the high temperature; this is a very important diagnostic point. In each of these cases the pulse was 80° to 86° when the temperature was 101° F. and higher; another symptom is the intense headache met with in this disease. As Surgeon-General BRUCE says, one cannot form valuable deductions from a few cases; we have only had four cases, but I thought our experience might prove of interest.

MORPHIA INJECTOR'S SEPTICÆMIA (WHITMORE'S DISEASE).

BY

A. C. STEVENSON, M.B., D.P.H.,

Wellcome Bureau of Scientific Research.

The specimens shewn illustrate some points of the pathology of a disease which is fairly common in Rangoon, and in certain ways resembles glanders, but which differs from that disease in there being no skin lesions and in the character of the organism causing it. The disease, first described by Major WHITMORE, was first noticed in a man who had the morphia habit. As it appears to be commoner in such people, it received the above name, but it is not confined to such cases.

Attention was drawn to the disease by the first post-mortem appearances seen—the nodules in the lung; but nodules may also be found in the liver, kidneys, and spleen, while the lungs may be free. When near the surface of the organ, these nodules shew as yellowish areas, surrounded by one of redness, and feel distinctly firm to the touch. On section, caseation is suggested by the appearance, but this practically never goes on to softening, except in a very small way.

Microscopically, the caseating area is seen to be made up of largely distended alveoli, filled with polymorphonuclear and mononuclear leucocytes and endothelial cells. In some areas these are seen to be necrotic. The red area is one of engorgement of the blood vessels and hæmorrhage into the alveoli.

Amongst, and in the leucocytes in the alveoli, small beaded bacilli are seen. They do not retain Gram's stain, nor are they acid fast. Their length and the number of beads in them vary considerably. When cultivated on ordinary media they appear as short rods, with generally two dark staining dots in them; on salt agar they grow into long filaments. They are motile in the early stages of cultures, but this soon disappears.

Inoculation into guinea-pigs invariably leads to a fatal result with the formation of nodules. If only a small dose is used, ½ to 1 minim of an 18 hours' broth culture, intraperitoneally, enlargement and inflammation of the testicle is got, as in Strauss' sign in glanders, in about 36 hours. With large doses death ensues too quickly. Guinea-pigs are also capable of infection by feeding with cultures.

A full account of the disease and its pathology can be found in an article by Major WHITMORE in the *Journal of Hygiene* of April, 1913, to which I am indebted for what knowledge I have.

The specimens and the parent culture of these shewn was a present to the Bureau from Dr. L. G. FINK, of the Rangoon General Hospital.

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ANATOMICAL DATA CONCERNED IN OPERATIONS ON THE LIVER AND IN CLINICAL EXAMINATIONS.

BY
JAMES CANTLIE, M.B., F.R.C.S.

The so-called paper I am about to bring forward, is really a demonstration of some anatomical and clinical points, which are better given upon the blackboard and body than by way of a paper. Nothing is more variable than the relation of the liver to the chest wall in health, and the extent of the variation is multiplied in disease.

We are often content to say when estimating the extent of the liver area, that it falls short of the cartilages of the right side of the chest and extends so many fingers' breadths below it. Variations under these conditions obtain, however, in health as well as in disease.

There is no fixed anatomical standard by which to gauge these relations, for everyone has his (or her) own chest features as pronouncedly as he has distinguishing facial features and aspect.

In a short thick-set man with a broad chest, the cartilages of the ribs slope away in an almost transverse or horizontal direction, exposing

a much larger area uncovered by rib cartilage than a tall narrow-chested man, in whom the ribs descend so low, and slant outwards so gradually, that the liver may scarcely be felt, nor present dulness on percussion beneath them anywhere, and certainly not in the nipple line. I need not labour this point, as everyone knows it to be the case.

Seeing then that we possess unreliable data as regards the clinical relationship of the liver to the thoracic and abdominal walls, I attempted to find a ready clinical method of determining a standard measurement to go by.

According to anatomical teaching, the lower border of the liver in health occupies a line drawn from the cartilage of the ninth rib on the right side, to the cartilage of the eighth outer left, that is to say, the line of percussion dulness of the liver rises from the ninth right cartilage to the eighth left. This statement affords little or no help, and certainly is devoid of any real attempt at precision.

These cartilages are both of considerable length, as they taper upwards to their ending, and to determine which is positively the eighth and ninth cartilage on the living body is beyond the power of the most skilled anatomist. The measurement is mere guess work, and is of no value clinically; for if the line be taken from the lower part of these cartilages instead of the upper, it may mean that there is an inch or more liver substance present, than if the line is taken from the centre or upper ends of these cartilages; but one inch added to the bulk of the liver would mean about 1lb. more weight of liver, or an enlargement to the extent of one quarter of its entire bulk.

Now there are only one or two clinical constants in determining the size of the liver. One—and it is the most patent—is as follows:—the upper border of the liver in the middle line of the body is on a level with the xipho-sternal articulation, an anatomical landmark which can be always determined. The liver cannot rise above this point, for there is the central tendinous leaflet of the diaphragm, which practically never varies in position, which we know to be the upper limit of the liver in health and disease. This fact is of little help in the matter of percussion, for the hepatic dulness runs upwards into the cardiac dulness, and it is impossible to determine which is which by percussion. Instead of this, however, by placing the front of the *patient's right hand* across the chest with the knuckles on a level with the xipho-sternal

ridge, and the upper border of the hand held horizontally, the lower border of the hand will exactly correspond to the lower border of the liver in health. By pressing the hand firmly against the body, the bulge of the hypothenar eminence on the lower border of the hand will make a slightly wavy outline which corresponds to the lower border of the liver in this region.

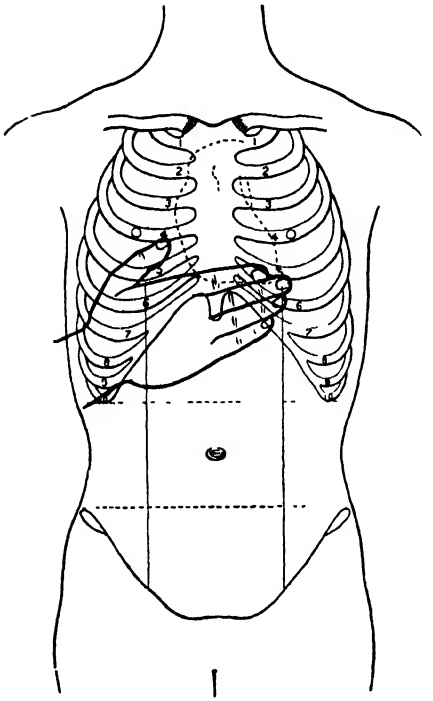

Fig. 1.

Shewing how the lower border of the patient's right hand laid across the chest, with the radial border of the forefinger parallel to the xipho-sternal articulation, corresponds to the normal lower border of the liver. Dulness on percussion falling short of, or reaching lower than, this line indicates decrease or increase on the size of the liver.

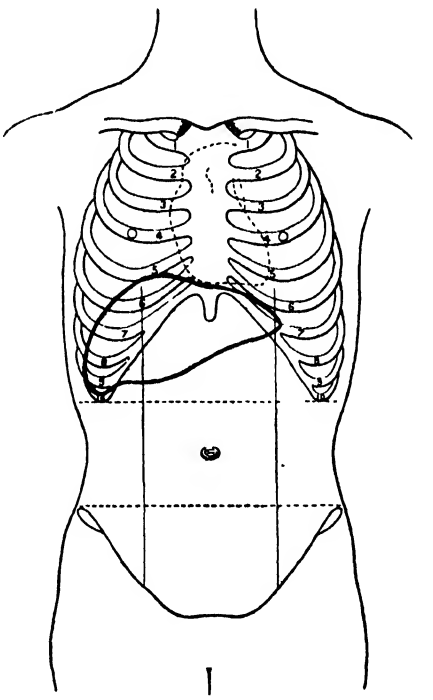

Fig. 2.

Showing the normal outline of the lower border of the liver.

If a line is drawn along the lower border of the hand thus placed, subsequent percussion or palpation, or as I prefer the tuning-fork-stethoscope method, will shew immediately whether the liver dulness

falls short of or extends beyond the normal line, indicating either an atony or an hypertrophy of the liver.

This is really the only constant anatomical point; but there is another which is of some account. The highest point of the liver dulness is mid-way between the sternal border and the nipple line; from thence it descends outwards until at the nipple-line level it is $\frac{1}{2}$-inch lower and at the mid-axillary line $1\frac{1}{4}$-inches lower; and, further back still, descends rapidly as far as the interval between the tenth and eleventh ribs. When the liver is enlarged (or pushed upwards), its upper border becomes more horizontal, or may become quite horizontal as far as the right axillary line, or it may be even still further back. This is a clinical feature of some help in diagnosis, but less exact and constant than the previously named.

When we come to the liver itself, there are many points of anatomical importance, which I will endeavour to explain, and I will begin at the beginning of how I became possessed of the knowledge.

No textbook ever written hinted even that there is any difference in size between the right and left branches of the portal vein, of the right and left branches of the hepatic artery, nor of the right and left hepatic ducts. Yet we read that the left branches go to the left lobe and the right branches to the right lobe. But the right lobe is treble the size of the left and it seems an anomaly to me, as it must to anyone, when it is pointed out that the smaller left lobe has the same sized vessels (arteries, veins and ducts) as the right. I must say this never occurred to me until I met with the following case:—It was that of a post-mortem upon a Chinese prisoner, who committed suicide by hanging in the jail at Hong Kong. I mention the cause of his death in order to show that he died of no hepatic ailment, but cut short his own life whilst in good health. At the post-mortem the liver presented the following anomalous appearances. The right "side" (I use the word advisedly in preference to "lobe") of the liver existed as a mass of fibrous tissue, and looked like, and practically was, a mere appendage to the left side of the organ. This fibrous mass was adherent to the diaphragm immediately above it; from hence, through the diaphragm and lung tissue to a large bronchus near the root of the lung an extension of the fibrous tissue shewed clearly that a liver abscess had occupied the right side of the liver, and that it had burst upwards through the diaphragm and lung

into the bronchus. The completeness of the destruction of the right side of the liver was such that nothing was left to shew that the liver cells or vessels had ever existed in the area. With the whole of the right side thus obliterated, a marked change in the nature of a hypertrophy had developed on the left side. The increase in appearance made the parts seem elephantine almost, and the weight of the liver was but a few ounces below that of the normal liver. Inspection of the organ, however, brought a new feature clearly home to one. The part which struck one most as regards bulk was the lobulus spigelii, which protruded as a thick, blunt mass of liver tissue. And not only this minor lobe, but the lobulus caudatus, from which it sprang, and the lobulus quadratus in front, also shewed this enormous hypertrophy. The hypertrophy of the left side joined with the atrophied right side, shewed the gall-bladder in the front and the inferior vena cava at the back on the right of the liver. On dissection, the vessels, veins, artery, and duct of the right side were obliterated, and those of the left side were increased in diameter proportionately to the increase of the bulk of tissue which they supplied or drained.

The only explanation of this pathological state was that an abscess had destroyed the right side of the liver, and that the left hypertrophied and supplied its place and function, so that the man was, hepatically, in as good a state as before his right side had been destroyed by the abscess.

In the course of my reading I came across various statements such as the following:—"The immunity of the left lobe of the liver, not alone from cancer, but from abscess, as well as from hydatids, is a pathological phenomenon to me quite inexplicable. No doubt an anatomical or physiological cause for its immunity must exist, but what it actually is I know not."—(HARLEY). This remark was called forth by a case recorded by the same author, with reference to which he says:—"But little hepatic tissue remained (in the right lobe); yet, notwithstanding this, and the fact that the diseased growths were secondary to a similar morbid degeneration in the sigmoid flexure, the left lobe of the liver, in spite of being enlarged, was free from disease."

The fibrous state of the right lobe in the Chinaman whose case I have cited cannot be called unique, for at the post-mortem in the

Middlesex Hospital, on a case of Dr. GOODFLLOW'S (also recorded by Dr. HARLEY), it was found that, whilst the right lobe was studded with cancerous nodules, the left lobe was atrophied, and looked like a mere appendage to the right, not exceeding 1½-inches in diameter. Several other cases could be quoted bearing on the same point, but the above will suffice.

In elucidation of this condition, I venture to submit various observations taken from time to time, the large majority of which were communicated at the meeting of the International Anatomical Society in Dublin in 1896.

Experiment I.

A. The first experiment was merely to examine closely the size of the right and left vessels met with at the portal fissure. The branches of the portal vein could only occasionally be thus gauged, owing to the variability of the seat of its bifurcation; but when the two main branches part some little distance before touching the liver substance, the two divisions measure practically the same in every instance.

B. The hepatic artery, by its earlier bifurcation, is more easily dealt with. After the giving off of the branch to the gall-bladder from the right hepatic branch, the diameters of the two vessels approximate each other very closely—in fact, are practically identical.

C. The hepatic ducts, as they converge to form the common hepatic duct, are also of the same size.

Experiment II.

A. The next experiment was merely that of making an incision from before backwards through a normal liver, from the fundus of the gall-bladder to the centre of the spot where the inferior vena cava grooves the back of the liver; that is between the points which mark the line of division between the halves (not the right and left lobes) of the liver. The weight of the two masses thus divided approximated each other within a drachm or two.

Experiment III.

With this before one, a further investigation led one to injections of the various vessels, and with the following results:—

A. Injections of the right and left divisions of the portal vein, with different coloured substances, shewed that the injected areas met along

a line, leading from the centre of the notch corresponding to the gall-bladder, to the notch for the inferior vena cava. *(See Figs. 3, 4 and 5.)*

It was not possible to inject the right from the left vessels, nor *vice versa.*

The line where the areas meet correspond to the weight line.

B. Injection of the hepatic artery might, *a priori*, have been expected to afford similar proofs of separation of the right and left sides. The injection does not, of course, yield such patent and easily observed results as in the case of the vein injections, but it was evident that the left artery usually supplied the accessory lobes to the right of the antero-posterior fissure—in fact, that at the mid-line the areas injected along the hepatic arteries meet.

C. A similar, but as precise, conclusion was arrived at in the case of the hepatic ducts.

The anatomical evidence seems, therefore, to point to a completely separate vascular supply up to the mid-line of the liver; that line being situated, as above stated, not along the antero-posterior fissure (that for the ductus venosus and umbilical vein), but along a line drawn from the centre of the notch for the gall-bladder upon the anterior border of the liver, to the notch for the inferior vena cava at the posterior margin. The injections shew that the left branch of the portal vein is distributed, not only to what we call the left lobe of the liver, but also to the minor lobes and to the liver substance of the so-called right lobe, up to a line drawn from a point over the fundus of the gall-bladder to the groove for the inferior vena cava. The same applies to the distribution of the left branch of the hepatic artery and the left hepatic duct. There are, therefore, practically two livers, a right and left liver, which lie in juxta-position, but are independent in their vascular and bile duct supply. *(See Figs. 3, 4 and 5.)*

Clinical Evidence.—Perhaps in malignant disease, more than in any other, is it observed that the disease is confined to one lobe.

A. Dr. PYE SMITH, in the 31st volume of the *Pathological Society's Transactions*, quotes a case where circumscribed masses of malignant material were limited to the right lobe of the liver.

B. Dr. EWART, in the *British Medical Journal*, September, 1860, refers to a case in which the left lobe was free from cancerous material,

whilst the right was occupied by a malignant mass weighing 120 ounces.

C. Dr. WEST quotes a case of a child, eight months old, with a huge hepatic tumour, which turned out to be the almost healthy left lobe pushed out of its place by the diseased right lobe.

D. In no case, however, is confinement to one side more apparent than in hydatid disease.

E. The tropical abscess of the liver is confined to one lobe of the liver so much so that it is pronounced "a single" abscess. However large it is it never crosses the barrier between the right and left side, so that, however big the liver abscess, one half of the liver remains intact, but develops in size sufficient to carry on the work of a normal liver even when one lobe is completely destroyed.

Developmental.—The liver is at first an exactly symmetrical organ. The organ arises in the form of two diverticula of hypoblast, which grow from the ventral wall of the duodenum immediately beyond the stomach. The two diverticula become the right and left hepatic ducts, and around them the right and left lobes subsequently attain an equal size.

The common bile-duct, although seemingly indicating an asymmetrical development, does not affect the question, as it is formed, not out of a coalescence of the hepatic ducts, but is formed later by a protrusion of that part of the duodenal wall with which the original diverticula are connected.

The apparent asymmetry of the gall-bladder and cystic duct is in the same category, for the lumen of them is formed by a diverticulum from the common bile-duct, itself an asymmetrical tube. And, as in several animals—horse, mouse, etc.—no gall-bladder exists, the consideration of the gall-bladder and its duct may be left out of the question.

It would appear, therefore, that the liver is a symmetrically developed organ from the first; and that it should become asymmetrical afterwards—that is to say, have one side larger than the other, that is, a large right and a small left lobe—is not in harmony with the development and growth of any other organ in the human body.

Surgically.—The liver, when split or fissured by a blow, as between the buffers of railway-carriages, splits along the mid-line of the liver in preference to any other.

From these considerations, it would seem that the liver is a sym-

Fig 3

Injection of the right branch of Portal Vein shewing the lobulus quadratus and half the area beneath gall bladder unaffected The slope of the liver in the photograph partly obscures the fact that neither the lobulus spigelii nor the lobulus caudatus are injected

Fig 4

Injection of the left branch of the Portal Vein shewing its distribution not only to the left lobe but also to the minor lobes

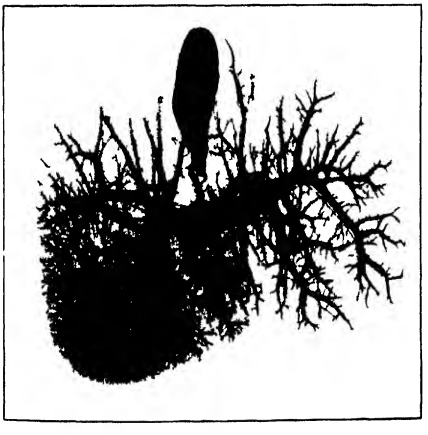

Fig 5

Injection of both branches of the Portal Vein, shewing minor lobes supplied by the left branch

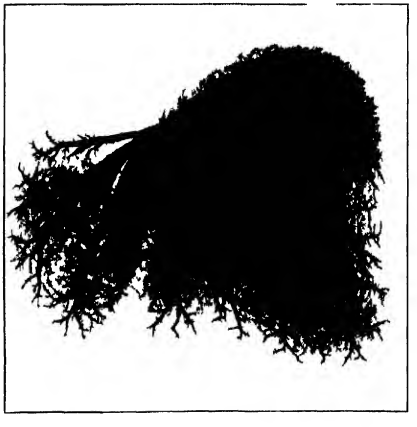

Fig 8

Injection of the Hepatic Vein, shewing that there is a left and right grouping of the veins as they collect to join the Inferior Vena Cava

Illustrations to accompany Mr J Cantlie's Paper

metrical organ in the adult, its symmetry to be gauged not by the antero-posterior or longtitudinal fissure (that for the umbilical vein and the ductus venosus), but along a line drawn from before backwards through the fundus of the gall-bladder to the spot where the inferior vena cava grooves the back of the liver. In other words, that we have two lobes (or two livers), which coalesce along a mid-line, giving a right and left half, the left half including the minor lobes, the lobulus quadratus, the lobulus spigelii, and the lobulus caudatus.

In other words, that the gall-bladder occupies a position mid-way between the lobes, and lies in the groove separating the two original halves. Further, that the inferior vena cava also takes advantage of this interval, and travels upwards between the halves.

The practical bearing of this has yet to be proved surgically; but there can be no doubt that any surgical interference with the liver will be much more readily tolerated as it approaches that line, which I have termed the mid-line of the liver, and that the hæmorrhage has less to be dreaded as the liver is incised or torn in the neighbourhood of that line.

It is theoretically possible to tie the vessels of one side at the gate of the liver, supplying an abnormal growth in one or other of the liver lobes, leaving the other side to do the work.

That one-half of the liver can hypertrophy, so as to perform the function of the whole, is attested by pathological study, in the same way that one kidney can develop so as to carry on the work of the two.

There are many other anatomical points of importance in dealing with the surgery of the liver.

Of these may be mentioned:—

Danger from Hæmorrhage.—The danger of searching for pus in the liver by a hollow needle consists in the fact that it has been attended by hæmorrhage of even a fatal character. It is unfortunate that such an untoward accident should have occurred, as it is apt to prevent practitioners adopting the only means we know of by which to make certain that pus is in the liver. To obviate this accident occurring, I made some observations whereby the danger may be minimised. The vessel in danger of being wounded is the inferior vena cava. Hæmorrhage from the portal vein of a serious character would occur were the

needle to puncture the vessel *before* it reaches the liver substance, but such an accident, although not impossible, is so improbable as not to be worth considering.

Hepatophlebotomy.—When the needle enters a branch of the portal or hepatic vein in the liver substance the operator should fill the syringe, not only once but again and again, until, say, 6 or 8 ounces of blood are withdrawn. This conduces to a relief of the hepatic congestion and to a lowering of temperature, if it is above the normal, in a manner more decisive than any other known therapeutic means.

The puncture of the capsule of the liver by, say, half-a-dozen insertions of the needle, tends further to relieve tension within the organ; the escape of blood from these punctures further tends to relieve the congestion—the blood escaping into the cavity of the peritoneum where it becomes absorbed. That hæmorrhage from these punctures does actually occur I was able to ascertain with certainty. The occasion was the tapping of the abdominal cavity for ascites. Whilst the ordinary clear ascitic fluid was escaping through the cannula I inserted a needle into five different places in the liver, which was immensely enlarged. Quickly the clear fluid escaping through the cannula became tinged with blood, and finally appeared to be so markedly hæmorrhagic that the cannula was withdrawn, and the abdomen firmly bandaged. No untoward sequel occurred, but on the other hand great relief was afforded to the patient, who expressed himself greatly pleased at the result of the treatment. We know, therefore, that the liver does bleed when punctured, and in this way is explained the great benefit that follows simple puncturing of an inflamed liver, even when no pus is found during the search. The only vessel in danger's way during the operation of liver-needling, therefore, is the inferior vena cava. I undertook some measurements in regard to the depth of this vessel from the surface, with the idea of obviating the danger of wounding it during puncture of the liver. The following facts were adduced: (1) The inferior vena cava lies at (practically) equal distances from the surface of the chest wall anywhere in the region between the lower end of the sternum in front and the angle of the lower ribs on the right side, that is, the region of operation in liver-needling *(Figs. 6 and 7)*. (2) Given a chest of 32 inches circumference at the seat of operation, the centre of the trunk of the

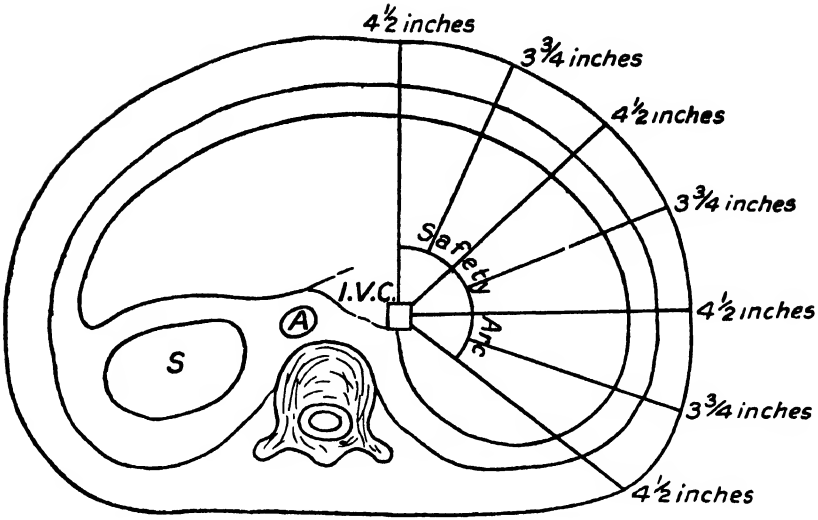

Fig. 6.

Transverse section of Body through area of liver. *I.V.C.*, Inferior Vena Cava. *A*, Aorta. *S*, Stomach.

The circumference of the body over the liver area was 32 inches. The lines marked 4½ inches shew equal distances from the chest wall to the *I.V.C.* The lines marked 3¾ inches shew the depth it is safe to puncture a liver by a needle. These lines reach the safety level at the "safety arc."

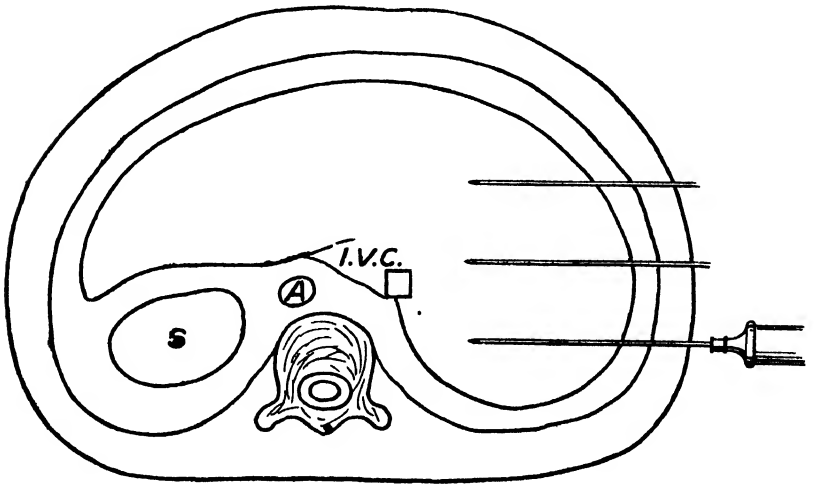

Fig. 7.

Shewing the directions the needle should be introduced into the liver; thus avoiding the needle point being always directed towards the *I.V.C.* By this means also the liver is more thoroughly searched.

inferior vena cava is (practically) everywhere in the area mentioned 4½ inches distant from the surface. (3) In a chest of greater or less circumference than 32 inches the inferior vena cava is deeper or more less shallow respectively, but in what exact proportions I have not yet had opportunity of determining. (4) It follows, therefore, that in a chest of 32 inches circumference at the level of the liver the puncture-needle should not be introduced more deeply than 3¾ inches, so as to ensure not puncturing the inferior vena cava.

HEPATIC PUS FOLLOWS WHEN IT PASSES UPWARDS INTO THE CHEST.

ANATOMICAL POINTS IN THE DIRECTION.

I will not enter at length into the question of the division of liver abscesses into supra-hepatic, intra-hepatic or sub-hepatic abscesses. Nor will I refer to the suggested etiology of these varieties, nor to the treatment I allocate for each of these. These points have been dealt with elsewhere, and they have been generally accepted in the textbooks, so need not be repeated here.

There is one point, however, of anatomical interest which I should like to draw attention to. When pus from the liver bursts upwards it may reach one of four media on the way to the surface :—

(a) It may pass through the diaphragm, and passing beneath the pleura reach the intercostal region low down in the chest.

(b) It may burst into the cavity of the pleura.

(c) It may, after causing a basic adhesive pleurisy, pass into the lung substance, and find exit by a small bronchus within the lung substance. It is interesting here to note that the channel from the liver to the bronchus is never direct, but, instead, the pus permeates the lung tissues, scattering through the lung as water in a marsh, and like as a rivulet exudes from the marsh the pus gathers in the bronchus to find exit.

(d) The pus may find its way upwards, between the inner aspect of the right lung and the pericardium, and between the layers of the ligamentum latum pulmonis to reach the bronchus as it emerges from the root of the right lung. This ligament is apt to be forgotten by the clinician, but it plays an important part in the guidance of pus towards the lung root.

This is the most dangerous channel of all for liver pus to take, for it means that a great depth has to be reached before the channel can be tapped. Attempts have been made to reach it from the front by removing the cartilages of the right ribs in front, but, owing to extensive adhesions between the inner surface of the right lung and the pericardium, the attempt had to be abandoned. I have twice reached it successfully from the right side between the ribs by a needle, but only after removing parts of two ribs (without opening the pleura), and *then* inserting the needle. The depth is so great—usually seven inches from the surface—that, were a needle made of that length, there is danger of so long a needle breaking during the operation. I, therefore, by removing ribs, saved at least 1½-inches in the needle length.

It is of but little use leaving this form of abscess exit to time to cure. If it comes through the lung tissue, cure will, in all probability, occur in time; but the exit by the way of the ligamentum latum pulmonis resembles more of the nature of a test tube, with its bottom on the liver, its walls thickened by inflammation, and its mouth above at the bronchus. There is little or no possibility of this closing by natural methods, and an operation whereby the lower end of the test-tube-like channel is reached has to be undertaken, in spite of the danger of penetrating the pericardium and, it may be, the heart.

On the occasions on which I have done this operation, I introduced the needle until I struck the hard and thickened wall of the channel and penetrated it with anxiety, in case I had mistaken the channel wall for the pericardium.

Discussion.

The Chairman (Sir David Bruce): In thanking Mr. Cantlie for his interesting paper, I may say that nothing of this was taught in the Anatomy Class of the University of Edinburgh, when I was a student. I wonder if this dual nature of the liver, which has been so clearly, so vividly and so convincingly presented to us by Mr. Cantlie, is still untaught in modern anatomy. I envy and congratulate the discoverer of this important anatomical fact on his quick powers of observation in being able to deduce from an area of old scar tissue the existence of a right and left liver.

Professor SIMPSON: I listened with very great pleasure to Mr. CANTLIE'S lecture. I am only sorry I was not a student under Mr. CANTLIE, he makes things so clear and plain. His observations are always very suggestive, and that one about the two lobes being two livers is new, and certainly, I think, a very important one, and very satisfactory for those patients who have got liver abscess if it is only confined to one liver. Of course it is easy to be wise and to know after one has been told. The suggestion of there being two livers is supported by the fact that it conforms to the general mechanism of the body. There are two brains, two lungs, two kidneys, two ventricles, and so on, and there is no reason whatever why there should not be two livers.

I am grateful to Mr. CANTLIE for the way in which he has demonstrated to us how to diagnose the exact position of the liver. It has always been somewhat of a trouble, and certainly the method he has shewn us is very simple, and one hopes to profit by it.

Mr. J. CANTLIE: With regard to the question you ask concerning my teaching of the liver anatomy being accepted, I may say one eminent Professor of Anatomy said lately he always taught it to his students since 1896, when this matter was before the International Congress of Anatomists. It was there received in solemn silence, and the Chairman said: "I wish you had told us what you were bringing forward, and then we would have looked the matter up." I am aware it will take another generation or two before this theory will be generally taught.

AN INTERESTING CASE OF SYPHILITIC PYREXIA IN AN INDIAN NATIVE. THE VALUE OF A POSITIVE WASSERMANN REACTION IN DIAGNOSIS.

BY

GEORGE C. LOW, M.A., M.D.,

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London School of Tropical Medicine.
Joint Secretary Society of Tropical Medicine and Hygiene, etc.

I bring forward the following case and shew the charts of the same, as it is a most instructive one from the point of view of diagnosis. Febrile disturbances in natives are often specially difficult, because in many cases one can only obtain the most meagre history, and therefore has to depend largely on physical signs alone. It is often only after a prolonged study, where all the laboratory tests have been called into play, that one is able finally to say, and even then there are cases which recover with no diagnoses having been made.

The following are the notes of the case. The temperature charts I have put on the table, where any interested may study them at their leisure :—

Sheik, Alli Mali, aged 25, male. Born in Calcutta; East Indian. Lascar on board "ss. Nyanza," P. & O. Co. Admitted into Seamen's Hospital, March 14th, 1916, where he was found to be suffering from pneumonia of the left lung lower lobe. As he could not speak English, little or no history could be obtained from him. It seemed, however, that he had been definitely ill for three days before admission.

Systems.—Apart from the condition of the lung, there was little abnormal to make out. His teeth were good, tongue clean. Lungs: right, normal; left, consolidated at base, whole of lower lobe. Heart: no murmurs, ordinary size. Abdomen: pigmented mark over region of liver; had evidently been painted with iodine here on some previous occasion; organ slightly enlarged downwards, edge very hard. Spleen,

9 to 11. Nothing palpable in abdomen. Urine: no alb. blood or sugar. Knee jerks ordinary; no scars about legs; penis, nothing to note.

Blood.—No malaria or other parasites.

Fæces.—Ova of ascaris, trichocephalus, ankylostomes and oxyuris present. Protozoa negative.

Progress.—The disease progressed favourably; temperature fell on the 18th to normal, and resolution, though slow, began in the usual manner. On the 1st of April, a rise of temperature to a 100° F. took place in the evening; as it went up next night again, patient was treated with eucalyptus ɱxxx., chloroform ɱxlv., castor oil ʒx. on the 5th of April (ʒv. at 10 a.m. and 11 a.m.), on the assumption that the helminths were causing the temperature. No ankylostomes were recovered, but some adult oxyuris came away. A leucocyte count shewed 10,500 whites per c.mm.

From now onwards the temperature began to swing and take on a hectic type, up in the evenings to 103 F., or thereabouts; down in the mornings to sub-normal. An examination of the lung revealed crepitations still at the left base, so the area was painted with strong iodine to promote absorption. On the 17th of April Dr. WHITEHEAD put an exploring needle into the bases of both lungs with negative results; no pus. The little sputum there was, though carefully examined, shewed no tubercle bacilli, and there were no signs of malaria in the blood.

It was then thought that there might be an abscess of the liver, but as the organ was in no way enlarged nor tender, this was put out of court. The temperature certainly pointed most to tubercle, or next to some septic condition or pus formation, but nothing localising could be found. There were no rigors, practically no sweating, and no cough.

On the 1st of May a complete blood count gave the following result:—

R.	4,900,000	
W.	12,500	No malarial parasites.
Hb.	100	per cent.
Differential—		
P.M.N.	60	per cent.
L.M.	6	,,
L.	33	,,
E.	1	,,
T.	0	,,
	100	,,

On the same day Dr. WHITEHEAD, while examining the heart, detected a slight roughening of the first sound over the pulmonary area, and the following day a distinct murmur became apparent in the same position. Subsequent examinations confirmed this and a thrill was also made out, while the patient complained of pain over the cardiac area.

On the 10th of May Dr. NEWHAM, while examining blood in the wards of the hospital for WASSERMANN'S reaction, amongst others, tested the patient's and found that it was strongly positive.

On the morning of the 11th, a mixture of liq. hydrarg. perchlor., ♏. 30; pot. iodide, grs. 10; aq. ad ℥ss., was prescribed thrice daily, the result on the temperature being of a wonderful nature, this falling to normal at once and remaining so for good. The leucocytes now numbered 11,250 per c.mm.

As it is very unlikely that the patient will go on with his treatment on returning to sea, Dr. NEWHAM gave him in addition two intravenous injections of galyl, with no bad effects.

Professor HEWLETT (*The Medical Magazine*, April, 1904, Vol. 13, No. 4, p. 267) and Dr. NEWHAM (*Journal of the London School of Tropical Medicine*, Vol. 2, p. 199) have already drawn attention to similar cases of pyrexia in tertiary syphilis with visceral lesions seen in the wards of the Seamen's Hospital, and I remember one not mentioned by the latter, namely, the case of a man who had an enlarged liver and a hectic temperature, with drenching sweats.

The abdomen was opened on the assumption of a liver abscess, but a gumma only was found. Hg. and KI. quickly caused the temperature and symptoms to disappear.

The interesting point about the present case is that there was no definite gross lesion indicative of syphilis, unless the cardiac condition and slight enlargement of liver could be looked upon as such. It is quite possible that the former was associated with a syphilitic aortitis, which probably accounted for the temperature. After the treatment the heart lesions cleared up distinctly, and now seem to have disappeared entirely. The liver is still easily palpable, slightly tender, and very hard.

In cases, then, of doubtful pyrexia, especially in tropical cases, it is clear that a WASSERMANN reaction should never be neglected, as it may, just as it did in this case, give the clue to an otherwise puzzling mystery.

It is interesting to note that in this case the leucocyte counts were

not of much help. They were increased a little, especially so if we take 7,000 as normal; but one has seen pus with counts as low as this; a case of liver abscess I can remember only having 9,000 white cells per c.mm.

Discussion.

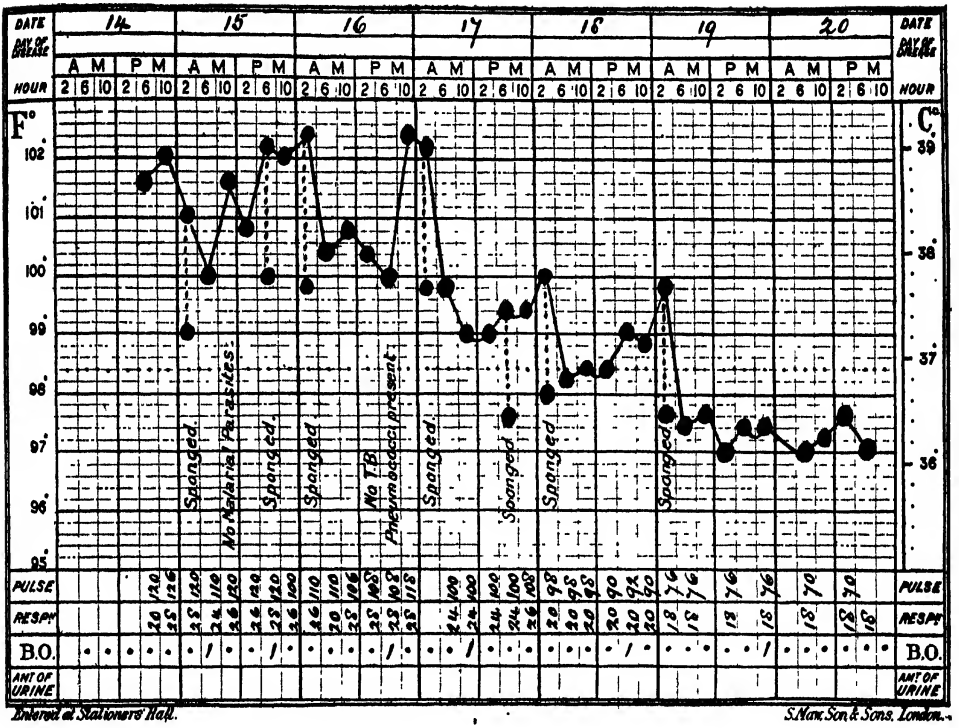
Professor Simpson: What stage was this syphilis?

Dr. G. C. Low: The case was a tertiary one, with, as far as one can judge, visceral involvement. Secondary syphilis has, of course, a certain degree of pyrexia, but this is quite different to the type of temperature described above in the present case.

Professor Hewlett and Dr. Newham have also made a special note of this in their papers which I have already alluded to.

Captain McGrigor: In answer to Mr. Cantlie's question as to the relative value of kharsivan and galyl, I cannot give a definite opinion yet, as the number of cases is not large enough to form an opinion upon to compare the two. So far we have only done a few hundred galyl injections, but up to the present it seems to be that kharsivan is the better product of the two. You give it in the same way as salvarsan. Galyl has to be given in weekly doses, which involves more visits to the physician than is the case with kharsivan. The general opinion is that kharsivan is the better preparation, in fact there appears to be no difference between kharsivan and the original German salvarsan.

MARCH, 1916.



Temperature during pneumonic attack.

APRIL.

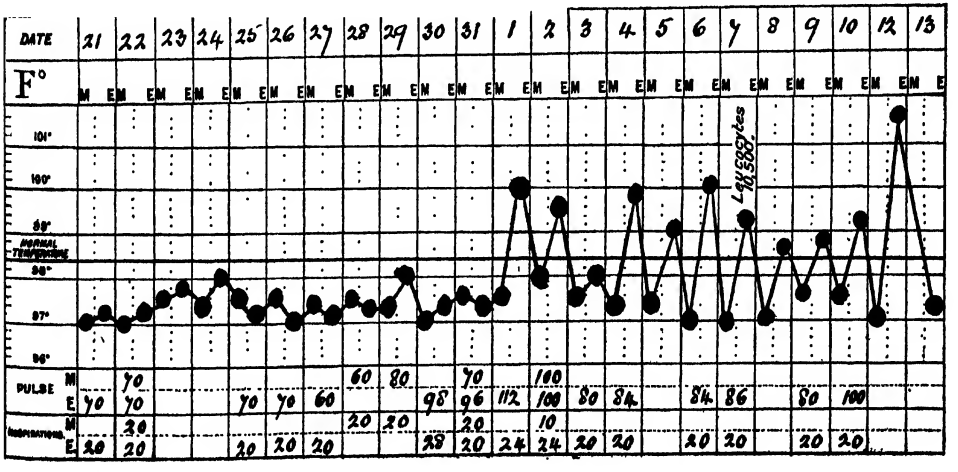
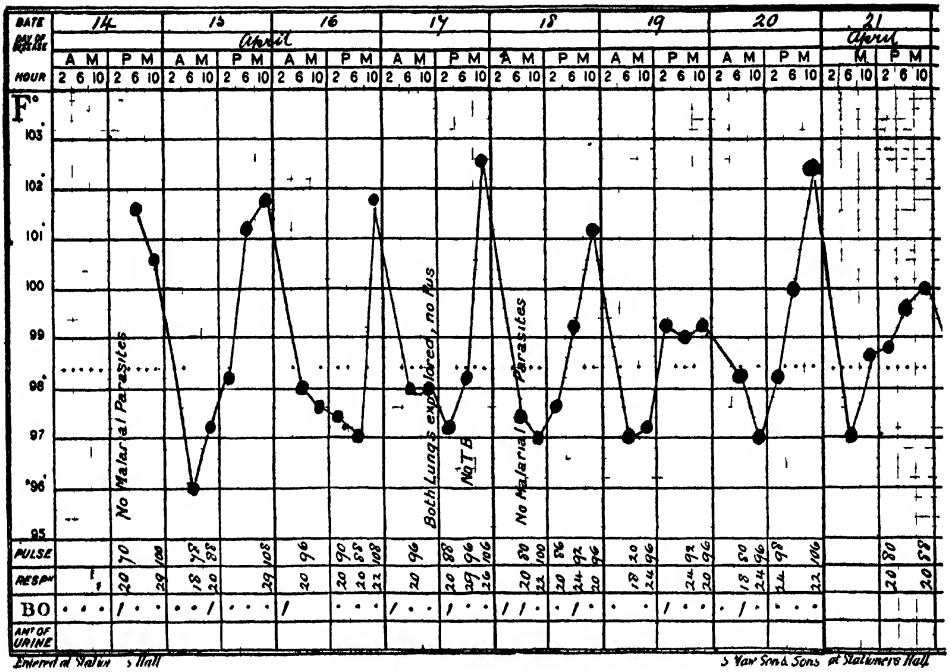
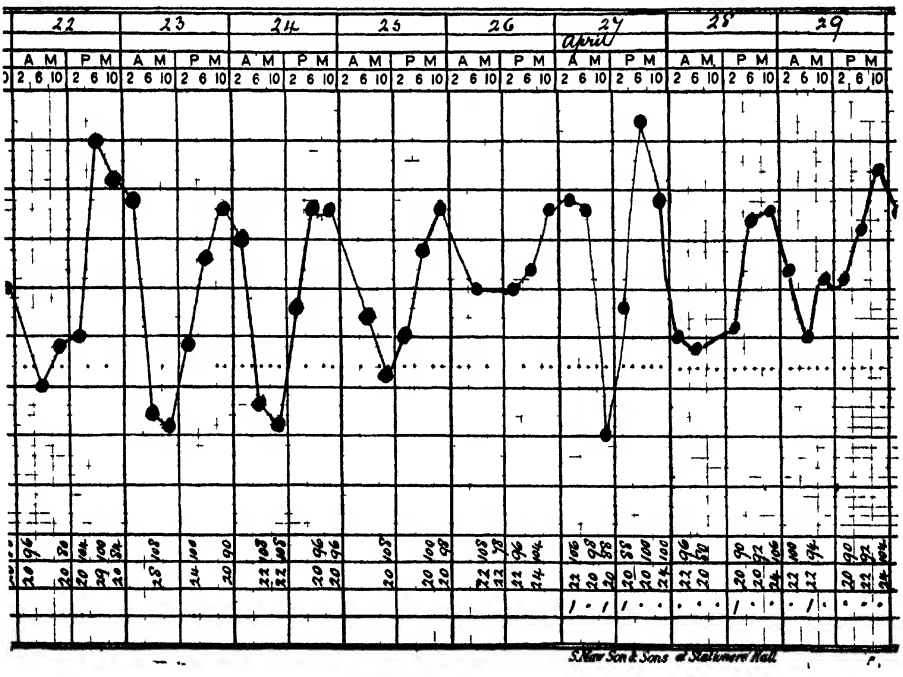


Chart shewing where the syphilitic pyrexia began.


DATE
DAY OF DISEASE
14 15 16 17 18 19 20 21
April
A M P M
HOUR
2 6 10 2 6 10
F°
103
102
101
100
99
98
97
96
95
No Malarial Parasites
Both Lungs consolidated, no Rus
No Malarial Parasites
PULSE
RESP
BO
AMT OF URINE
Entered at Station Hall
S Mair Son & Sons at Stationers Hall


22 23 24 25 26 27 28 29
April
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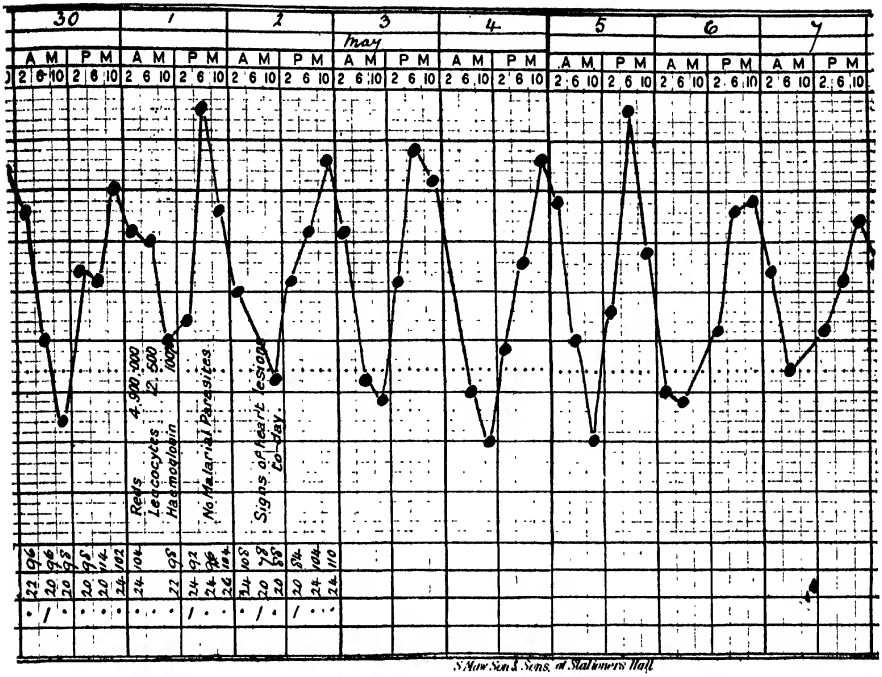
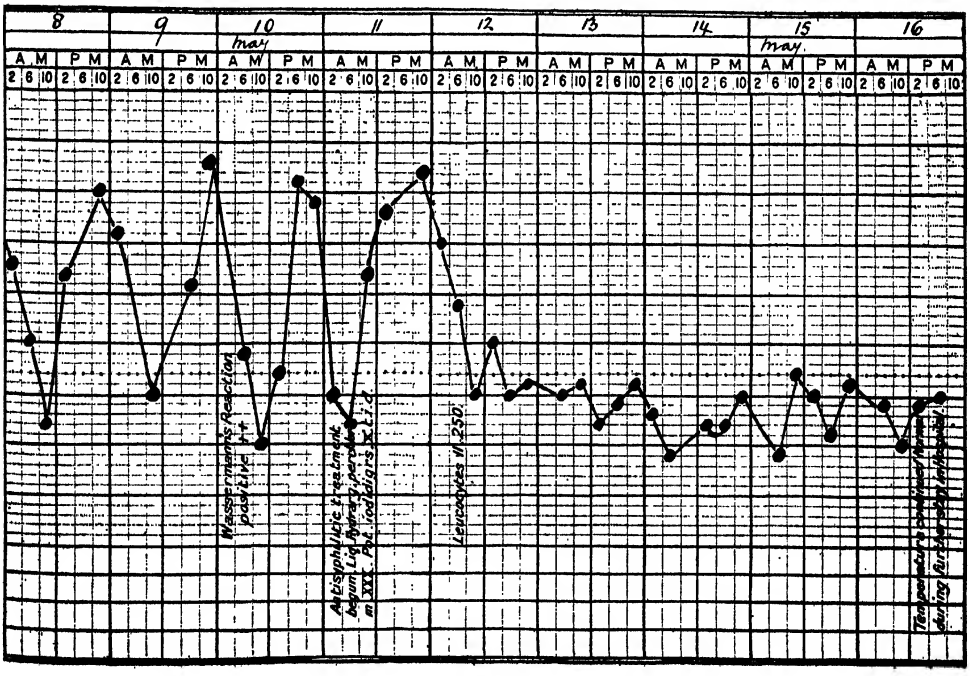
S Mair Son & Sons at Stationers Hall


30
1
2
3
4
5
6
7
May
A M
P M
2 6 10
Reds 4,320,000
Leucocytes 2,500
Haemoglobin 100%
No Malarial Parasites
Signs of heart lesion
Co-Say.
S. Maw Son & Sons, at Stationers Hall


8
9
10
11
12
13
14
15
16
May
Wassermann's Reaction
positive ++
Antisyphilitic treatment
began Liq. hydrarg. perchlor.
m XXX. Pot. iodidi gtts. X t.i.d.
Leucocytes 11,250
Temperature and pulse normal during first week of treatment

EIGHTH ANNUAL REPORT OF THE COUNCIL.

Chief Events of the Year.

Owing to the European War, which broke out in August, 1914, the past Session has necessarily suffered. At a meeting of the Council of the Society, held on October 16th, 1914, it was unanimously resolved that the work of the Session should continue, the papers being, as far as possible, devoted to subjects connected with the War. Cholera, Typhoid, Dysentery and other diseases of interest were therefore dealt with. Some of the later meetings, however, were poorly attended, Fellows apparently finding it difficult to be present.

Fellows.

During the year, 44 candidates were admitted as Fellows, making the total 918 since the formation of the Society. As a result of deaths and resignations this number has been reduced, but there are at the present time 676 Fellows on the books of the Society. The Council record with regret the deaths of one of their Honorary Fellows, Professor Angelo Celli, and of the following Fellows:—Lieut.-Col. W. Sandilands Harrison, Dr. Elizabeth McBean Ross, and Lieut. H. S. Ranken, M.B., R.A.M.C.

The Headquarters of the Society.

The Society has continued to occupy the room rented from the Medical Society of London, at 11, Chandos Street, Cavendish Square, London, W., on the first of January, 1914; this is open from 10 a.m. to 5 p.m. daily for the use of Fellows either for reading or for the examination of microscopic specimens, microscopes belonging to the Society being available for this purpose. The Library of the Society is housed in this room and the exchange publications are kept for the use of Fellows. A telephone has been installed so that Fellows may ring up to make enquiries at any time during office hours, or use the telephone when using the reading room.

The Library.

The Library has continued to grow, and a considerable number of journals and works have been bound and added to it, for which additional bookcases have been provided. It is hoped that Fellows will present copies of their works and any tropical literature which is not yet in the possession of the Society. A card index of the reprints in the possession of the Society has also been made, and these are classified and kept in cases so as to be readily available for use. The following Fellows have made valuable contributions to the Library during the past year:—Sir Havelock Charles, Dr. Sandwith, Dr. Wenyon, Dr. Harford and Dr. Low.

Papers and Demonstrations.

The following papers were read at the various meetings during the year:—"Mosquito Work in Ceylon," by Major S. P. James, M.D., I.M.S.; "Classification of the African Trypanosomes Pathogenic to Man and Domestic Animals," by Surgeon-General Sir David Bruce, C.B., A.M.S.; "Thymo-Benzol in Bilharziasis" (communicated), by William Robertson, M.D.; "The War and Typhoid Fever," by Sir William Osler, Bart., M.D., F.R.S.; "Tropical Problems in the New World," by Andrew Balfour, C.M.G., M.D.; "Bacillary Dysentery," by Dr. H. S. Gettings; "The War and Cholera," by Professor W. J. Simpson, C.M.G., M.D.; "Some Interesting Drugs of Tropical Origin," by Frank Lee Pyman, D.Sc., Ph.D.; "An Epidemic of African Tick Fever among the Troops in British Somaliland," by R. E. Drake-Brockman, M.R.C.S.; "Four Cases of Bilharziasis under Thymo-Benzol Treatment," by Dr. C. M. Ekins; "The Treatment of Ankylostomiasis in Venezuela," by C. E. F. Mouat-Biggs, D.T.M. & H.; "Buba, or *Leishmaniasis americana*, in Paraguay," by Dr. L. E. Migone; "Further Investigations on the Etiology of *Leishmaniasis americana*," by Dr. L. E. Migone (the latter five papers were all communicated); and "Some observations on Malaria on Rubber Estates," by the late R. M. C. Linnell, L.R.C.P., D.T.M. & H., B.A. (whose death has been recorded in the Transactions, Vol. VIII., No. 7).

TRANSACTIONS, PUBLICATIONS, ETC.

The Council have done their utmost to secure that the TRANSACTIONS of the Society shall be of special value to the large number of Fellows who are abroad, and who are kept in touch with the Society by this means. They cordially invite Fellows to communicate to them any new discoveries which they may have made, or any papers on matters of special interest, which they are willing to offer to the Society. In certain cases such papers may be read at meetings of the Society, but in other cases they may, at the discretion of the Council, be inserted in the TRANSACTIONS as special contributions. Notes and comments are also inserted which may be of general interest to Fellows.

The Council have decided not to issue a List of Fellows in the Annual Year Book this session, owing to the fact that the addresses of so many Fellows are at present uncertain, and accuracy in compiling the work could not therefore be assured. Fellows might therefore keep the last Year Book issued for the session 1914 to 1915 by them, as this may probably have to serve until the war is over.

It is particularly requested that Fellows coming home on leave, or returning to England, will at once notify the Secretaries of their home addresses. If in London they may call at the rooms of the Society, at 11, Chandos Street, Cavendish Square, W., and enter their names in the address book kept there.

Failure to give a definite address often results in the TRANSACTIONS and other notices sent out by the Society being returned by the Post Office, with "address unknown," "gone away," or "on leave" marked upon them.

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LOCAL SECRETARIES.

During the year Local Secretaries have continued to render valuable assistance in extending the usefulness and influence of the Society, and Fellows resident in districts which are as yet unrepresented by Local Secretaries who are willing to take an active part in the work of the Society in this capacity are invited to communicate with the Joint Secretaries.

CHARLES F. HARFORD,

GEORGE C. LOW.

Treasurer's Report.

(1) In the Financial Year—March 31st, 1914, to March 31st, 1915—the income of the Society was £569 14s. 11d., as compared with £667 13s. 9d. last year. The decrease in income is owing to the fact that subscriptions have not been paid with as great a regularity as in the previous year, and it is to be hoped that Fellows will note this and pay their dues as quickly as possible, as otherwise it is difficult to keep the books and the financial business of the Society up to date.

(2) The expenditure has been noticeably less, especially on the Transactions, this being partly due to the fact that one number had to be omitted (making seven publications instead of eight as usual).

(3) The Society has increased its balance from £287 11s. 5d. to £370 17s., £150 of which is now on deposit account at the bank.

The accounts have been audited and found correct by the Auditors, Dr. Beddoes and Dr. Bagshawe, to whom the thanks of the Society are due.

JAMES CANTLIE,

Treasurer.

FINANCIAL STATEMENT FOR THE YEAR ENDING MARCH 31st, 1915.

Income.	£	s.	d.
Balance from March 31st, 1914	287	11	5
Subscriptions...	480	14	9
Interest on Deposit for part year on £150	3	6	0
Advertising in TRANSACTIONS ...	70	9	8
Sales of TRANSACTIONS	15	4	6
	£857	6	4

Expenditure.	£	s.	d.
TRANSACTIONS Account	270	4	3
Rent, Epidiascope, Insurance, Refreshments	78	14	0
Clerical Assistance	78	0	0
Postages	20	6	0
Printing and Stationery	20	4	9
Pellagra Grant	13	14	10
LIBRARY—			
Books and Binding, Cabinet Index, etc.	5	5	6
	486	9	4
Balance on Deposit Account	150	0	0
Cash Balance at Bank	220	17	0
	£857	6	4

Audited and found correct this 14th day of May, 1915,

T. P. BEDDOES,
ARTHUR G. BAGSHAWE, } *Auditors.*

JAMES CANTLIE,
Treasurer.

NINTH ANNUAL REPORT OF THE COUNCIL.

Chief Events of the Year.

Owing to the continuance of the War the past Session has necessarily suffered. At a meeting of the Council of the Society, held on October 15th, 1915, it was resolved that the work of the Session should be continued, if possible. The time of the meetings was changed to the afternoon (5.30 p.m.) in the hope that more Fellows would find it convenient to attend at that hour.

Fellows.

During the year, 19 candidates were admitted as Fellows, making the total 937 since the formation of the Society. As a result of deaths and resignations this number has been reduced, but there are at the present time 651 Fellows on the books of the Society. The Council record with regret the deaths of Sir Francis Lovell, Professor E. A. Minchin, Major W. B. Fry, R.A.M.C., Captain M. F. Reany, I.M.S., Captain J. D. E. Holmes, Dr. James G. Gibb, Dr. A. C. H. Dickman.

The Headquarters of the Society.

The Society has continued to occupy the room rented from the Medical Society of London, at 11, Chandos Street, Cavendish Square, London, W., on the first of January, 1914; this is open from 10 a.m. to 5 p.m. daily for the use of Fellows either for reading or for the examination of microscopic specimens, microscopes belonging to the Society being available for this purpose. The Library of the Society is housed in this room and the exchange publications are kept for the use of Fellows. A telephone has been installed so that Fellows may ring up to make enquiries at any time during office hours, or use the telephone when using the reading room.

The Library.

The Library has continued to grow, and a considerable number of journals and works have been bound and added to it, for which additional bookcases have been provided. It is hoped that Fellows will present copies of their works and any tropical literature which is not yet in the possession of the Society. A card index of the reprints in the possession of the Society has also been made, and these are classified and kept in cases so as to be readily available for use. The following Fellows have made valuable contributions to the Library during the past year :— Sir Havelock Charles, Dr Sandwith, Dr. Leiper, and Dr. Low.

Papers and Demonstrations.

The following papers were read at the various meetings during the year :—"Pellagra considered from the point of view of a Disease of Insufficient Nutrition," by Dr. F. M. Sandwith; "Combined Vaccinations," by Professor Aldo Castellani; "An Interesting Case of Eosinophilia," by Dr. G. C. Low; "Intestinal Parasites in Northern Siam," read for Dr. A. F. G. Kerr; "The Causation of So-called Tropical Anæmia," read for Dr. W. M. Strong; and "A Method for the Trapping of *Glossina morsitans* suggested for Trial," read for Dr. J. O. Shircore; "The Etiology of Typhus," by Dr. W. J. Penfold; "The Sanitation of a Small European Settlement in Portuguese East Africa: with Notes on some of the Diseases Prevalent in the District," by Dr. R. M. Macfarlane, was printed in the March Transactions (no Meeting being held in February); "Experiences in the Treatment of Syphilis in the Army, with special reference to the Administration of '606' in Concentrated Solutions," by Captain H. J. McGrigor, R.A.M.C.; "The Treatment of Syphilis in African Native Tribes and among Native Workers on the Rand," by Dr. H. Bayon; "Cerebro-Spinal Fever, with the Notes of some cases," by Dr. J. M. Atkinson; Dr. A. C. Stevenson shewed some Specimens of a Lung from a case of Morphia Injectors' Septicæmia (Whitmore's Disease), and Cultures of a Bacillus isolated from the same; "Topography of the Liver in Relation to Liver Abscess," by Mr. James Cantlie, and "An Interesting Case of Syphilitic Pyrexia in an Indian Native: the Value of a Positive Wassermann Reaction in Diagnosis," by Dr. G. C. Low.

TRANSACTIONS, PUBLICATIONS, ETC.

The Council have done their utmost to secure that the TRANSACTIONS of the Society shall be of special value to the large number of Fellows who are abroad, and who are kept in touch with the Society by this means. They cordially invite Fellows to communicate to them any new discoveries which they may have made, or any papers on matters of special interest, which they are willing to offer to the Society. In certain cases such papers may be read at meetings of the Society, but in other cases they may, at the discretion of the Council, be inserted in the TRANSACTIONS as special contributions. Notes and Comments are also inserted which may be of general interest to Fellows.

The Council have again decided not to issue a List of Fellows or Annual Year Book this session, owing to the fact that the addresses of so many Fellows are at present uncertain, and accuracy in compiling the work could not therefore be assured. Fellows might therefore still keep the last Year Book issued for the session 1914 to 1915.

It is particularly requested that Fellows coming home on leave, or returning to England, will at once notify the Secretaries of their home addresses. If in London they may call at the rooms of the Society, at 11, Chandos Street, Cavendish Square, W., and enter their names in the address book kept there.

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CHARLES F. HARFORD,

GEORGE C. LOW.

Treasurer's Report.

(1) In the Financial Year—March 31st, 1915 to March 31st, 1916—the income of the Society was £578 19s. 2d., as compared with £569 14s. 11d. last year. The increase has been principally due to the satisfactory way subscriptions have been paid this year.

(2) The Society has increased its balance from £370 17s. to £442 1s., £300 of which is now on deposit account at the bank.

The accounts have been audited and found correct by the Auditors, Dr. Beddoes and Dr. Bagshawe, to whom the thanks of the Society are due.

JAMES CANTLIE,

Treasurer.

FINANCIAL STATEMENT FOR THE YEAR ENDING MARCH 31st, 1916.

Income.

		£ s. d.
To Balance March 31st. 1915 ...	£220 17 0	
Deposit Account	150 0 0	
		370 17 0
Subscriptions		513 5 5
Interest for one year on £150, and part year on £150 (total invested, £300) ...		6 4 11
Advertising in TRANSACTIONS		47 5 0
Sales of TRANSACTIONS		12 3 10
		£949 16 2

Expenditure.

		£ s. d.
By TRANSACTIONS Account		285 0 6
Rent, Epidiascope hire, Refreshments and Insurance		80 2 9
Assistant Secretary's Salary		78 0 0
Postages		18 10 0
Printing and Stationery		9 4 0
Telephone Subscription		6 10 0
Pellagra Grant		11 1 2
Subscriptions refunded + Bank charges of 2s.		3 5 0
LIBRARY—		
Bookcase		12 16 0
Binding		3 5 9
Balance at Bank (31st March, 1915)—		
Deposit Account	£300 0 0	
Current Account	142 1 0	
		442 1 0
		£949 16 2

JAMES CANTLIE,
Treasurer.

Audited and found correct this 12th day of May, 1916,

T. P. BEDDOES,
ARTHUR G. BAGSHAWE, } *Auditors.*

Date of issue.	Date of issue.	Date of issue.